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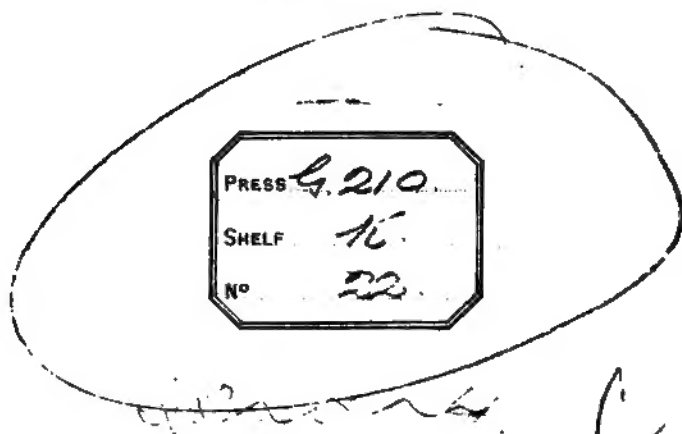
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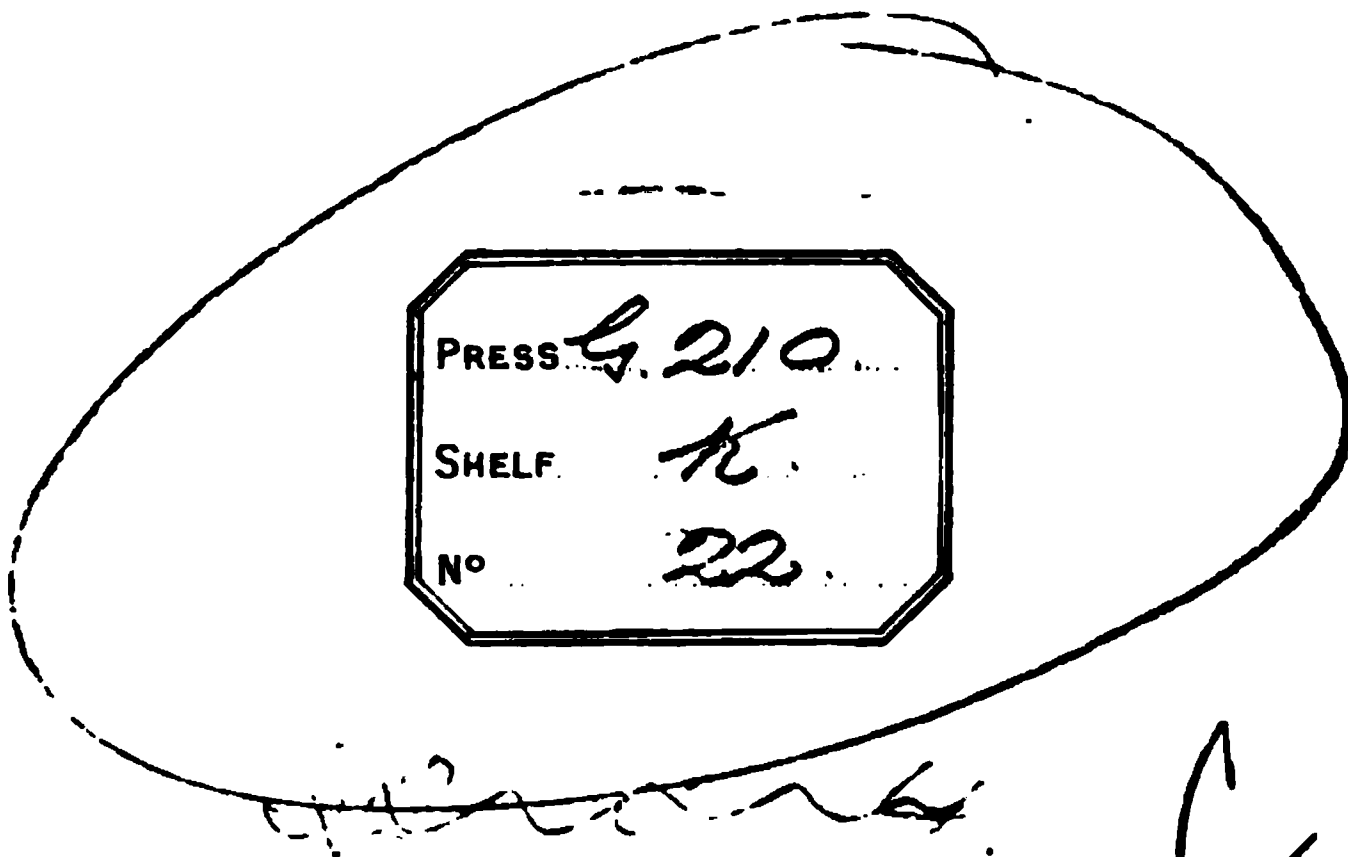




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**LIFE**  
**OF**  
**WILLIAM CULLEN, M. D.**





AN  
ACCOUNT  
OF THE  
LIFE, LECTURES, AND WRITINGS

OF  
WILLIAM CULLEN, M. D.

PROFESSOR OF THE PRACTICE OF PHYSIC IN THE UNIVERSITY  
OF EDINBURGH.

BY  
JOHN THOMSON, M. D., F. R. S. L. & E.

PROFESSOR OF MEDICINE AND GENERAL PATHOLOGY IN THE UNIVERSITY  
OF EDINBURGH.

IN TWO VOLUMES.

VOL. II.

COMMENCED BY  
DR JOHN THOMSON AND DR WILLIAM THOMSON,  
AND CONCLUDED BY  
DAVID CRAIGIE, M.D., F.R.C.P.E., F.R.S.E., &c. &c.

WILLIAM BLACKWOOD AND SONS,  
EDINBURGH AND LONDON.

MDCCCLIX.



TO

SIR BENJAMIN COLLINS BRODIE, BART.,

SURGEON TO THE QUEEN, PRESIDENT OF THE ROYAL SOCIETY, &C. &C.

GLASGOW COLLEGE, 1st August 1859.

*MY DEAR SIR BENJAMIN BRODIE,*

*It gives me sincere pleasure to be permitted to dedicate to you the concluding volume of my father's Life of Cullen.*

*This I do, not merely as a mark of the respect which I, in common with all scientific men, bear towards you, but also because I know that the high estimation in which my father held your scientific and professional attainments and researches, and the value he attached to your personal friendship, would have rendered this dedication peculiarly agreeable to him.*

*Believe me to be,*

*MY DEAR SIR BENJAMIN,*

*Most respectfully and sincerely yours,*

*ALLEN THOMSON.*



## NOTICE BY THE EDITOR.

---

IN now bringing before the public, after an interval of twenty-seven years, the concluding part of Dr Thomson's Life of Cullen, the writer of the present notice feels called upon to say a few words in explanation of the cause of the long delay, and of the circumstances attending the completion of the work.

From the year 1832, when the first volume was published, down to 1841, when Dr Thomson resigned his Chair in the University of Edinburgh, his leisure time had been employed, in concert with his eldest son, in preparing materials for the completion of Cullen's Biography; and in the course of 1841, about three hundred pages of the present volume were printed under their joint revision. The progress of the work then suffered a temporary interruption, in consequence of Dr William Thomson's removal to the University of Glasgow, which took place in the autumn of 1841. Their labours being subsequently resumed during a visit of Dr Thomson to Glasgow, about one hundred additional

pages were printed in 1844. Of this part it may be proper to state that the sheet marked 2 B, comprising pages 385 to 400, was only printed in proof, and not thrown off, and has since undergone a slight alteration.

The subsequent decline of Dr Thomson's health, his death in 1846, Dr William Thomson's increasing professional duties in Glasgow, and ultimately his illness and death in 1852, prevented the fulfilment of a design which both the authors had deeply at heart, and for the accomplishment of which they had laboured incessantly during many years.

The whole of the papers and materials connected with the work having passed into the hands of the present editor, he felt a strong desire that it should be brought to a conclusion and published, not only on account of its value as a contribution to Medical Biography and History, but from its interest to himself and to many others in connection with its authors. He was, however, too well aware that his own pursuits had not been of a nature to qualify him for such a task; and he felt impressed with the belief, that few men were to be found in the medical profession possessed of the amount of erudition requisite to secure accuracy in the narration, and fairness in the judgment, of the events to be treated of in the concluding part of this memoir.

He regarded himself most fortunate, therefore, when he succeeded in persuading Dr Craigie to undertake the task, feeling assured that his rare learning in the general history of medicine, and his special acquaintance with many of the topics discussed in this volume, qualified him in a peculiar manner for carrying out with success the design of the previous authors. It is to be understood, then, that the latter part of the present volume, from page 401 onwards, is entirely the work of Dr Craigie. To this the present editor has contributed nothing beyond revising the sheets and making suggestions as to the materials to be employed, and the plan to be followed in completing the volume.

At the same time it is proper to state, that much had been done by Dr Thomson and his son towards the completion of the work, by the collection of materials, the arrangement of a plan of treatment, and even by the composition and printing in proof of considerable portions of the remaining part of the volume. But the present editor, in concert with Dr Craigie, became aware, in reviewing these materials, that to have carried out in its full extent the plan indicated by Dr Thomson's writings, would have swelled the work to such proportions as, they conceived, would be unsuitable in the altered circumstances in which it was now placed. They felt, too,



that as years had passed on, while many of those were gone who would have taken an interest in the history of events and the description of doctrines with which they had been contemporary, the changes of modern medical science itself necessarily diminished in a great degree the value of the discussion of bygone speculative opinions in the estimation even of the learned reader of the present day. They accordingly felt the urgent necessity of shortening as much as possible that part of the volume which related to the history of Italian medical doctrines following upon Brunonianism. Should any imperfection or incompleteness, therefore, appear to belong to this part of the volume, this is to be attributed to the desire which the editor felt, that as much space as possible should be reserved for the biographical history of Cullen himself. With respect to the history of *Materia Medica*, in connection with Cullen's latest and important work on that subject, although Dr Thomson had planned, and even printed, a considerable amount of materials in regard to it, the same difficulty was felt from the want of space ; and Dr Craigie was under the necessity of rewriting nearly the whole.

With respect to the personal and private history of Cullen and his family, the editor and Dr Craigie have to regret the paucity of direct and accurate

information, arising from the removal by death, or absence of almost all those who could have supplied this history in greater detail. Even Dr Thomson had found the greatest difficulty in obtaining reliable information of this kind at a much earlier period. How much less prospect could there now be of satisfying the curiosity of the reader when the only grandson of Cullen is already approaching the age of four score years !

But however interesting these personal details might have been, it is as a history of Cullen's influence in promoting the advancement of medicine as a science and as a practical art that this memoir will possess its chief value. And the editor trusts that, notwithstanding the unavoidable defects which the work may possess, in consequence of the difficulties under which it has been completed, the part which is now given to the public may not only enhance the value of the first volume, by completing the biographical history of one of the greatest British physicians of the last century, but may form a useful and interesting contribution to the general history of medical science.

The editor takes this opportunity of expressing his grateful thanks to Dr Craigie for the zeal, diligence, and care with which he has endeavoured to verify all the facts and statements relating to Cullen's life and

other matters contained in the volume, and for the kindness with which he received any suggestions, which, in the character of editor, he has ventured to make as to the form of the work, and he hopes that the intelligent reader will agree with him in approving of the learning, candour, and judgment with which Dr Craigie has discussed subjects of no ordinary difficulty.

The editor has farther to state, that he has thought it proper to republish a short Biographical Notice of his father and brother. This notice is intended to be prefixed to the first volume, in consequence of the want of space in the present one ; but it may also be obtained separately from the publishers.

GLASGOW COLLEGE,  
*1st August 1859.*

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LIST OF ERRATA IN VOLUME SECOND.

- Page 30, line 20, insert "a" before "singular."  
 — 35, line 12, *for* "It it," *read* "It is."  
 — 69, line 12, *for* "Dialeses," *read* "Dialyses."  
 — 104, line 4, *for* "was," *read* "were."  
 — 134, last line, insert "s."  
 — 175, line 14, *for* "defintion," *read* "definition."  
 — 210, line 26, *for* "Ambrosioni," *read* "Ambrosiani."  
 — 227, line 30, *for* "of health and of disease," *read* "both in health and  
 in disease."  
 — 316, line 3, *for* "cessation from," *read* "cessation of."  
 — 342, line 11, *for* "hemphlegia," *read* "hemiplegia."  
 — 352, last line, *for* "reputation," *read* "refutation."  
 — 482, line 6, an "f" has dropped out at the beginning of the line.

**L I F E**  
**OF**  
**WILLIAM CULLEN, M.D.**

IN consequence of the arrangement with Dr Gregory which has been mentioned in the preceding volume of this narrative (p. 161), Dr Cullen delivered the course of lectures on the Practice of Physic, in the University of Edinburgh, during the winter-session of 1769-70. Previously to the commencement of that course, he published, in the Latin language, a Synopsis of Methodical Nosology, as a guide for those who were to attend his lectures ; thinking it, as he has himself remarked in the preface to the second edition of his Nosology, to be his first duty, in teaching the Practice of Physic, to instruct students of medicine as carefully as he could in the accurate diagnosis of diseases.

Nosology, regarded as a distinct department of medical science, calculated to facilitate diagnosis, was considered by Dr Cullen, as it had been by some of his predecessors in that line of investigation, to embrace three separate objects of consideration,—1st, The dis-



inction and definition of particular diseases, or of the genera and species of diseases ; 2dly, The nomenclature of diseases, or the assignment of the names by which they are to be designated ; and, 3dly, The arrangement or classification of diseases in some methodic and convenient order. The first of these objects of nosology, —the distinction and definition of particular diseases, —Dr Cullen regarded as being by far the most important ; and it was to its improvement that his efforts were at all times chiefly directed. The circumstance of his having been engaged, for a long series of years, in delivering lectures, first, on the Practice of Physic in the University of Glasgow, and subsequently on Clinical Medicine in Edinburgh, had necessarily served to direct his attention in a particular manner to the examination of those characters by which diseases may be most easily and accurately distinguished from one another, not only as an object of interesting speculation, and of immediate practical importance, but also as affording a means of facilitating the teaching and the study of medicine. And his intimate acquaintance with the rules of Logic, as well as the attention which he had bestowed upon the different branches of Natural History, by familiarising him with the principles upon which logicians and naturalists respectively distinguish different objects from one another, formed, when conjoined with the extensive practical knowledge of the phenomena of diseases which he possessed, the best possible preparation for an attempt to extend these principles to Nosology.\*

\* It is interesting to learn that, at the very time when the subject of Methodical Nosology in a particular manner occupied his

It appears from several passages in Dr Cullen's correspondence and manuscript lectures, that his attention was drawn in a very particular manner to the study of Nosology by the publication of M. Sauvages' *Nosologia Methodica*, at Montpellier, in 1763 ; for, soon after this period, he manifests, in his correspondence with his pupils, a desire to ascertain the opinions which other physicians had formed respecting the utility of a plan of Methodical Nosology, such as that followed in Sauvages' work. Thus, in a letter written by him in 1764 to his pupil Dr David Millar, who was then in London, he says,—“ Let me know if any body at London has read Sauvages' *Nosologia Methodica* ; or if any body enters into such a plan, or approves of others doing it ;” and it appears by a letter from his former pupil Dr Dobson of Liverpool, dated October 1764,

thoughts, Dr Cullen was engaged in studying, with great zeal, the characters by which minerals are distinguishable from one another. This appears from a passage in a preface, prefixed by the late Dr Walker, professor of Natural History in the University of Edinburgh, to a catalogue of the minerals contained in the University Museum. In the year 1764, Dr Walker made a tour through the Highlands of Scotland, by order of the General Assembly of the Church, for the purpose of inquiring into the state of the parochial schools in these districts. “ Not long before I set out,” says he, in the preface referred to, “ Dr Cullen had received the first German edition of Cronstedt's *Essay towards a system of Mineralogy*, of which he was so fond, that he carried it several weeks in his pocket. He translated to me out of it the leading characters of Cronstedt's new and peculiar classes. He was particularly anxious,” adds Dr Walker, “ about the Zeolite ; and it was in consequence of this that I first observed it among the basaltic rocks at the Giant's Causeway, and afterwards, in greater plenty and variety, in many of the Islands.” (See Appendix A.)

that the attention of that physician, likewise, had been directed to this subject by Dr Cullen about the same period. (See Appendix B.)

In a clinical lecture, delivered in January 1765, Dr Cullen gave a short but comprehensive view of the principles of Nosology, and an account of the labours of Sauvages in this department; and he continued, after this, to refer constantly in his clinical lectures to the definitions which that distinguished author had given of diseases, till the publication of the first edition of his own Synopsis. In the short course of lectures on the Practice of Physic which he delivered, with Dr Gregory's permission, in the summer of 1768 (vol. i. p. 161), he gave an introductory lecture on the distribution or arrangement of diseases, in which he mentioned his having printed a synopsis of the three methods of Sauvages, Linnæus, and Vogel. As this course was addressed to a class of advanced students, Dr Cullen found himself at liberty to enter on a comparison and critical examination of the higher divisions of Classes and Orders which these nosologists had established. In concluding this review, he remarks,—“These are the faults in the systems hitherto given. I must now offer you another. The matter is so difficult that I have hesitated much about making an attempt; but I hazard something to be useful, and an imperfect attempt is allowable here, though not fit to be offered to the public. As it is now given out, it is to be regarded as a sketch not finished.”

Into the first edition of his Nosology, published in the subsequent year (1769), Dr Cullen introduced the definitions of the genera of diseases that had been given

by Sauvages, Linnæus, and Vogel, together with the attempt he had himself made to simplify the classifications they had proposed, and to give fuller and more accurate definitions of at least the principal genera of diseases. In an address to his pupils, prefixed to this edition, after stating generally the objects and nature of the work, he remarked, that, notwithstanding the valuable labours of his predecessors in this department, it was not to be supposed that Nosology was yet in a perfect state, or that it would be improved in any other way than by repeated attempts and by much labour and time ;—that in the attempt which he had himself made to improve the characters of diseases, he had bestowed more pains on those of Universal, than on those of Local diseases, which latter class he had considered only in a superficial manner ;—and that he was aware he had left many things imperfect, but he hoped to be able to explain and to vindicate those matters which might appear incorrect or doubtful.

Two years afterwards (in 1771), when it was again Dr Cullen's turn to deliver the practical course, he published a second edition of his Nosology. Besides endeavouring, in this edition, to improve the definitions of the classes, orders, and genera of diseases, he added, under his own genera, a list of, and references to, the principal species enumerated by Sauvages ; and prefixed to his own part of the work a preface entirely new.

A third and carefully revised edition of Dr Cullen's Nosology appeared in 1780 ; but it is the fourth edition, published in two volumes 8vo, in 1785, which contains

the last corrections of the author, and exhibits his nosological labours in their most finished form. Into the third edition he introduced, besides the classifications of Sauvages, Linnæus, and Vogel, already mentioned, that of Sagar, as contained in his *Systema Morborum Systematicum*, published at Vienna in 1771 ; and into the fourth, that of Dr Macbride, as contained in his *Methodical Introduction to the Theory and Practice of Physic*, published at London in 1772. Numerous reprints of these different editions of Dr Cullen's *Nosology* appeared upon the Continent, particularly in Holland, Germany, and Italy.

Though Dr Cullen's *Synopsis* was, on the whole, very favourably received by the medical public, yet, by some very distinguished physicians, the study of *Nosology*, when first introduced by him into this country, was considered as a frivolous and unattainable pursuit. His favourite pupil, Dr *Monro Drummond*, in writing to him from London, 14th December 1771, says, " So far as I know, Sir John Pringle thinks the properties of diseases to be such as render them incapable of those methodical and strict arrangements which are applicable to plants ; and the modern *Nosology*, in consequence, fanciful and useless ; and not only so, but hurtful also, by fixing the mind on the circumstance of collocation merely, and detaching it from more accurate investigations into what is in general so little known, the thing itself to be placed. This I never heard him express myself in so many words ; but from what I have heard him say, and have learned from others, such, I collect, is the opinion he entertains. Ac-

cordingly, you may believe, what I have been otherwise told, that he has neither considered Sauvages' work nor yours with much care. Many of the other physicians here have heard of Nosology, but few have studied it. There are some botanists among them, but mechanical ones merely, for the most part, I suspect ; or if systematic, no practitioners,—like Solander,—and logic you know is out of fashion here."

These and other objections to the study of Nosology, Dr Cullen has carefully considered and ably refuted in the preface to the fourth edition of his Synopsis ; and also in an introductory lecture on the subject of Nosology, with which, for some years before his death, he was accustomed to open his course on the practice of physic. That lecture I have introduced into the edition of his Works which I published in 1827 (vol. i. p. 444), from a persuasion that few if any of those who have followed him in this department of medical diagnosis, or who have indulged themselves in criticising his labours, have fully understood or duly appreciated the objects that he proposed to himself, and the principles by which he was guided, in the composition of his Synopsis. It contains, I believe, a much abler exposition of the true nature, objects, and principles of Nosology, than is to be found in any other writing yet extant, except, perhaps, in the excellent Prolegomena with which Sauvages introduced his work to the knowledge of the medical public.

It is difficult to ascertain at what precise period medical men began to reduce the detailed histories or descriptions of diseases, to those brief enumerations of

their peculiar characters which constitute the first object of nosology, their definitions. That the state of disease is not simple, uniform, and identical, but manifold, and that its manifold character, as exhibited in the variety of external phenomena which different cases of diseases exhibit, depends partly on differences in the seat, and partly on differences in the nature, of the morbid affections to which the economy is subject, are facts which must have been perceived at a very early period of medical practice, and which could not fail to lead to the recognition of particular diseases.

The writings attributed to Hippocrates (some of which are universally allowed to have been the composition of that author himself, whilst others are evidently the production of some of his predecessors, contemporaries, and immediate successors or followers) shew that a very considerable knowledge of the external phenomena of particular diseases had been acquired at the period at which these works were written ; for we find in them surprisingly accurate descriptions of most of the diseases with which we are at present acquainted, under, in many instances, the identical names which they bear at the present day. “ Those who have collected what are called the Cnidian Sentences,” says Hippocrates (*De Victus Ratione in Acutis*), “ have well described the symptoms of diseases, such as they shew themselves, as well as the manner in which certain of them terminate.” In the Hippocratic writings, however, few if any definitions of diseases are to be found ; nor, indeed, do definitions occur

in any of the writings of the ancient physicians till a considerable time after the period in which Hippocrates lived.

From some of the few fragments which have been preserved of the medical authors who lived between the time of Hippocrates and that of Celsus, and from the testimony of later authors, it would appear that the practice of attempting to define diseases, sometimes by their supposed nature, and at other times by the concurrence and succession of their symptoms, had been introduced and followed, to a considerable extent, by some of the empirical physicians in Greece, and by Erasistratus, Herophilus, and their followers in Alexandria.

There is a small treatise, entitled *Ἐπεὶ ἰατρικῶν*, that has been published in several editions of Galen's works, though it is not now considered as the production of that author, which consists wholly, as its name imports, of definitions relative to different branches of medical science. Of nearly 500 definitions which it contains, upwards of 200 relate to particular diseases, and in several instances two or more definitions are given of the same disease; many of these possess a wonderful degree of accuracy. In a letter prefixed to this treatise, the author states that the ancient physicians, and those predecessors of Hippocrates whose writings had come under his notice, did not define any terms connected with the art. The only definition by Hippocrates to which he refers, is that of medicine itself, contained in the book concerning Art (*Περὶ Τεχνῆς*.) "Of the physicians who came after Hippocrates, some," he says, "wrote and employed definitions, and others did



not. The followers of Herophilus and Apollonius Memphites, and likewise Athenæus Attalensis, seem to have been diligent in this theory; but even they did not follow any regular order, nor did they compose a regular treatise, but introduced their definitions occasionally in their works." The author, therefore, undertakes the task of collecting the definitions given by the older physicians, arranging them in a proper order, and of supplying, himself, such as are deficient. It is singular that this remarkable treatise, which contains the earliest elements of Nosology, should have entirely escaped the notice of Sauvages, Cullen, and others, who, in modern times, have occupied themselves with framing nosological definitions and diagnostic characters of diseases.

Coelius Aurelianus, who was in all probability a contemporary of Galen, has introduced into his work on Acute and Chronic Diseases, a considerable number of definitions of diseases derived from medical writings of an earlier date than the period at which he himself lived, but which writings have not reached our times. In particular, he refers to a book or books of definitions by Asclepiades, who, as is well known, was the fashionable physician at Rome in the time of Cicero, two hundred years before the time of Galen, and the inventor of the methodic system of medicine. The comments which Coelius Aurelianus makes on several of these definitions, and the changes on them which he proposes, shew that he had examined the principles on which nosological definitions should be formed, with no inconsiderable degree of care.

In the descriptions of diseases given by Aretæus of

Cappadocia, who lived about, or perhaps a little previous to, the time of Galen, frequent use is made of definitions to mark their different species by proper diagnostic or pathognomonic symptoms. Alexander Trallianus, a medical author of the 6th or 7th century, is equally attentive to the definitions and the diagnostic symptoms of most of the diseases which he describes. It is curious to observe how minute and accurate both he and Celsus, who lived several centuries before him, were in their diagnostic distinctions of some of the cutaneous affections.

Subsequently to the revival of letters in Europe, the systematic writers on medicine seem generally to have given short definitions of the diseases of which they treated, along with an account of their etymologies, synonymes, and varieties. These definitions were, in the same work, sometimes derived from the supposed nature or proximate cause of the disease, sometimes from the symptoms manifesting themselves during its course, and sometimes from a combination of both. Examples of such definitions are to be found in the *Lilium Medicinæ* of Gordonius, which was dictated by that teacher to his students at Montpellier in 1305 ; and, at a later period, in the writings of Fernelius, Plater, and Sennertus. The practice, however, of giving short definitions of diseases, was in a much greater degree reduced to a system by Johnstonus in his *Idea Universæ Medicinæ*, published at Amsterdam in 1644, than it had been by any practical author previous to his time. To the account which he gives of each disease, this author has prefixed a definition of it, printed in italics, and quite separate from its history, so that the whole of

them might very easily be extracted and formed into a system of nosological definitions.

The great advantages derived, in modern times, in the study of Natural History, from establishing, upon fixed and determinate principles, definitions, not only of its various objects individually, but also of those more or less comprehensive groups under which it was found possible, from their analogies, to arrange them, suggested to medical men the idea that much advantage might be obtained in medicine from defining diseases, and such groups or families of diseases as they found it expedient to recognise, under general terms or common names, upon similar principles. Sydenham, whose sagacious and comprehensive mind seems to have been strongly impressed with the regularity of the courses and successions of symptoms which occur in the natural progress of diseases, notwithstanding the varieties produced in them by peculiarity of constitution, epidemic influence, and diversity of treatment, recommended that descriptions of diseases should be framed after the model of those that are given of plants, and he himself lays down various judicious rules for the proper execution of this object in the preface to his work on Acute Diseases, first published in 1675.

The precepts of Sydenham were approved of by Baglivi, and by several other medical philosophers; but it does not appear that any attempt was ever made to reduce them to practice, or to define and arrange diseases according to the principles adopted by botanists, before this idea was taken up and acted upon by Professor Sauvages of Montpellier. It is well known

that this distinguished physician published in 1732 the outlines of a nosological system, in which, arranging diseases under classes, orders, and genera, he endeavours to lay down the characteristic phenomena of each, as well as to enumerate their principal varieties. The very favourable reception which this work met with from the medical public, and particularly the approbation bestowed on it by Boerhaave, encouraged Sauvages to continue his labours in the new and interesting field of medical inquiry which he had begun to explore, for an additional period of thirty years, when he gave to the world, in two volumes 4to, his "*Nosologia Methodica*," a work which, whatever may be its defects or errors, marks an important era in the history of medicine, as having led to much greater accuracy in the distinction of diseases than was previously observed, and which cannot fail to secure a lasting reputation to the name of its author.

Dr Cullen, in assuming that the proper foundation for the distinction of particular diseases is the occurrence, in a number of persons, of steady and uniform combinations of morbid phenomena or symptoms, presenting themselves in concourse or in succession, had it for his principal object in the composition of his work on Nosology to mark each particular disease by such a short enumeration of its leading characters as might serve to distinguish it from every other ; or in other words, to establish a series of correct nosological definitions. It had then been urged by the opponents of Nosology, as it has frequently been since Dr Cullen's time, that diseases are too unsteady and variable in their characters, to admit of definitions of them being

framed in conformity with the principles observed in defining the objects of natural history. To this objection, however, Dr Cullen by no means gave his assent.

“ The distinguishing of things by genus and species,” he remarked, “ is *universally* applicable to every two things in nature that are capable of being distinguished from one another, and, therefore, among other things, to the distinguishing of *diseases* ; and this mode of distinction only requires that we should know what is peculiar to one thing and common to no other ; it is to know every thing by some circumstance or circumstances peculiar to itself. The several distinguishing circumstances or particulars, and the combination of them which serve this purpose of distinction, constitute what in Natural History we call a character ; in Physic we call it a Pathognomonic.” (Works vol. i. p. 446.) —“ That the distinction of diseases is sometimes difficult, is confessed by all ; but it must also be allowed that in some cases it is possible, for if any one were to deny this, it would be the same as to say that there is no science or art of medicine.” —“ At present the distinction of diseases lies in so much confusion, that Nosology must certainly be difficult, and some carry it so far as to assert that the attempt is impossible. But certainly this is going a great deal too far. I acknowledge that many difficulties occur in Methodical Nosology, and the difficulties are sometimes such as I cannot pretend to surmount ; but this is not universal ; if it were so, what would be the consequence ? It would make the study of physic absolutely impossible, for if we cannot arrive at some distinction of diseases, we must act at random. Nay, this is as much as to say that there are greater difficulties in our way in ascertaining facts, than all that have been urged against false theories.” —“ If medical men are able to distinguish diseases from one another, they must surely be able to tell by what marks they do so. But these marks can be none else than those which define each disease by genus and spe-

cies, which again can only be ascertained by a properly conducted nosological method. Nosology only tells you in what manner any person does distinguish diseases. I presume no person will deny that we can go a certain length in this, and certainly no objection can be urged against going a certain length, though we can go no farther—but how far we can go nobody can say till it has been tried. I have no hesitation, therefore, in affirming that the distinction of diseases, hitherto in many cases doubtful, may be rendered more certain by a nosological method. Besides, it is to be remarked, that, as often as we attempt to establish such a distinction of diseases, this advantage will arise, that either the deficiencies or the errors of our definitions will be very easily perceived, and, when perceived, will lead either to the more accurate consideration of observations previously made, or to a greater degree of accuracy in subsequent observations, and will thus be of much service both to Methodical Nosology, and in rendering the diagnosis of diseases more perfect.”

Those who are conversant with this subject will readily perceive how large a portion of the objections that have been recently urged against the study of Nosology, in the writings of M. Broussais, and of Drs Marshall Hall, Barlow, and others, has been anticipated and refuted in these remarks of Dr Cullen’s.

It has been much disputed among pathologists, whether the definitions of diseases should be derived from the external phenomena that present themselves in their course, or from the internal pathological conditions on which these phenomena are supposed to depend; and particularly such of these conditions as consist in structural alterations discoverable after death. Dr Cullen declared himself in favour of definitions derived from the former of these sources. It was for the purpose of guiding the formation of definitions founded upon

symptoms, not of superseding them, that he conceived the nosologist ought to make use of the information derived from pathological anatomy. So understood, he is quite consistent both with himself and with Sauvages, though, in the last of the passages about to be quoted, he seems to betray some suspicion of the contrary.

“It is, I think, now agreed,” says Dr Cullen, “that the dissection of morbid bodies is one of the best means of improving us in the distinction of diseases. Sauvages has, indeed, rejected the employment of the internal seats of diseases as a means of distinguishing them; but he has, in a hundred instances, tacitly employed it; and, under the ambiguity that often occurs in external symptoms, it is evident that dissection, by shewing the part singly or jointly affected, shews the real and steady changes in the system upon which the external symptoms depend, and, therefore, must lead to the proper limiting of genera and species;—and if in no other way, at least in this, by leading us to observe more exactly the external appearances connected with the internal that are observed by dissection. This, therefore, like the attempts in system which we mentioned before, must lead to a more accurate observation of particulars. On the present subject, I think it must appear evident that the distinction of diseases must be often guided by the dissection of morbid bodies,—must be constantly guided by anatomy, physiology, and pathology, united together.”

“By looking into the systems of medicine,” says he at another place, “you will perceive that physicians have gone on in the same track of defining diseases by their proximate causes, which are, in many cases, disputable, and may long be so. This we must avoid hereafter. We must endeavour to distinguish diseases independently of every theoretical view; for the theory we employ, however specious, however much we may be persuaded of its truth, will not appear in

the same point of view to others, and must, therefore, occasion endless confusion.”—“I have formerly expressed myself as if the internal seat might, in consequence of our now knowing it so well from dissections, frequently be attended to in forming our characters ; but I am satisfied that though this internal seat is, with much propriety, added to the history of the disease, it is better not to be taken into the specific character, as it is never directly evident, and only such marks ought to be employed as are evident.” (Works, i. p. 457, 8.

In the criticisms which Dr Mason Good has made on Dr Cullen's Nosology, in the preface to his own work on the same subject, he has stated as one of his objections to it, “the want of discrimination between genera and species.” “It is impossible,” he remarks, “to take a survey, however brief, of Dr Cullen's system, and not to notice his very extraordinary confusion of genera and species. And the author,” continues Dr Good, “is the more induced to advert to it, because, extraordinary as such a confusion must appear to all who are acquainted with the difference, Dr Cullen is by no means the only nosologist of our day who has run into the same mistake.” (P. 19.) “This charge against Dr Cullen,” Dr Good again remarks, “is, that while, in some cases, he has given genera with the proper species belonging to them, in others he has given genera without any species whatever ; and in others, again, he has described species under the name of genera.” (P. 21.)

That Dr Cullen was fully aware of the difficulty, and consequent inaccuracy, here pointed out by Dr Mason Good, is abundantly proved by the following extract from his introductory lecture on Nosology, in



which he has satisfactorily explained the circumstance which had led nosologists into the mistake of applying the designation of genera, in many instances, to what are properly species of diseases.

“ According to the principles of method, that is strictly called a species in nosology, which is a concourse of symptoms, or a character not admitting a farther subdivision of steady and constant particulars. The species, therefore, is the ultimate character ; and whenever you come to this, it ought, in strict propriety, to be called a species, and not a genus. But no system has avoided this difficulty or error. You will find that, in every system of nosology [and it may be added, of natural history] the name of genus and species are, in this respect, confounded. But the thing to be remarked is, that it is the ultimate concourse, the ultimate character, not admitting of subdivision unless into varieties, which you are to have in view in the study of diseases, no matter whether you call it species or genus. That is the character in nosology which is the fixed object of our practice, and the chief object; for though I know that the study of the varieties may be necessary, it is by no means universally so. And I would here observe, that though I have said that we can only aim at a generic practice, I now say more clearly that such genera as do not admit of farther division, may be considered as species. It has happened, I think accidentally, that in nosology, by beginning with the characters of the class, and from thence descending to orders and genera, the term genus has been very universally applied to species ; and in short, by imitation, I have been led to do the same thing; for of the 133 genera which I have established [in the first edition of the Nosology], 100 are properly species, and admit of no farther division, except into varieties. These genera, as there characterised, have the ultimate concourse, the steady character of species, and admit of no farther subdivision but into what are truly varieties. In the other systems of noso-

logy, the number of genera enumerated has amounted to a great many more : but I maintain that the same proportion holds ; that three-fourths, perhaps, of their genera are species, in the strict language of method. I have come to my conclusion, that the chief business in nosology is the formation of the character of what I have called genera, but what you truly, for the most part, may consider as species." (Works, i. p. 454.)

Dr Good, in order to avoid the mistake which preceding nosologists had committed in applying the term genus to the species of diseases, has, in his Nosology, instituted a number of entirely new genera, which he has designated by names sanctioned, as he conceives, by classical authority, but which certainly are not in use at the present time, and were never employed for the purpose for which he has thought fit to revive them. It would not be difficult to point out, in this portion of Dr Good's work, several inconsistencies with the best established rules of method, and to shew that some of the diseases which he has denominated genera are, in fact, species ; while others of them ought, according to the strict method of naturalists, to form orders, rather than genera. It may be doubted, too, whether the advantage to be gained in nosology by accuracy in the discrimination between genus and species, be commensurate with the inconvenience that must necessarily result from the introduction of so many additional names as Dr Good has employed, into medical nomenclature.

Ploucquet, in his *Delineatio Systematis Nosologici*, published at Tubingen in 1791-93, in four vols. 12mo, in styling Dr Cullen's definitions of diseases the flower of his work, has had the hardihood to allege (Præf. p. 23)

that they were for the most part borrowed from Vogel.\* In framing his definitions, Dr Cullen had before him the systems of Sauvages, Linnæus, and Vogel, independently of any help he might take from practical authors; and it must at least be allowed, that he afforded to every one an opportunity of judging how much he borrowed from the nosological labours of his predecessors, as well as what alterations or improvements he made upon their definitions, as he reprinted these along with his own, in order that, to use his own expression, "being placed together, they might the more easily be compared, and that, being compared, they might indicate the characters that are best or most suitable, or might lead the reader to correct such as are erroneous, and to obtain more exact distinctions." However much, therefore, or however little, Dr Cullen may have availed himself of the generic definitions of Vogel or of any of his other predecessors in nosology, no unfairness or want of candour can be attributed to his conduct in this respect. But on a comparison of Dr Cullen's generic definitions with those of his predecessors in nosology, there do not appear any reasons for believing that he employed Vogel's definitions more frequently than those of Sauvages and Linnæus, as the groundwork of his own; still less that he implicitly adopted any of them, without endeavouring to correct

\* It is not a little illustrative of the notions entertained by this very distinguished medical scholar respecting nosology, to find that his *Delineation of a Nosological System* does not contain a single definition; arrangement and nomenclature, with references to the writers on the different diseases, forming the sole objects of that work.

and amend them, as far as his knowledge or experience enabled him to do so. If, for example, the definitions which Dr Cullen has given of the diseases included by him under the different orders of his second class, the *Neuroses*, as those of apoplexy, palsy, syncope, dyspepsia, hypochondriasis, chlorosis, &c. be compared with the definitions of the same diseases given by Vogel, the incorrectness of Ploucquet's assertion will appear very manifest.

Another equally unfounded statement on the part of Ploucquet is to the effect that, "in marking the species of each genus, Dr Cullen for the most part merely divides the species of Sauvages into idiopathic and symptomatic, and has scarcely done anything which would make it worth while to copy his enumeration of species." Upon this statement, it is only necessary to observe that Dr Cullen has himself given, in the following passage of his introductory lecture, a candid and much more correct statement than that given by Ploucquet, of the use which he had made of Sauvages' divisions into species.

"You must begin by studying what we call genera; but you must not stop there: it is absolutely necessary to proceed to the study of species, and even to that of varieties; and I have therefore thought it proper to attempt giving you some assistance in this particular. I wished, indeed, to have ascertained and *characterised* the several species, as I have done under the genus *Cynanche*, according to my own view of the matter;\* but I found that, owing to various insupe-

\* It seems not undeserving of attention that the genus *Cynanche*, the several species of which Dr Cullen states he had ascertained and characterised according to his own view of the matter, is pointed out by Dr Good as one of the instances in which Dr Cullen has adhered to a regu-

rable difficulties, particularly as I had no other assistance but the nosology of Sauvages, this was impossible. I have attempted, however, to make my arrangement more convenient and useful than that of Sauvages. He has gathered together a great many particulars relating to species and varieties of diseases ; he seems to have marked down whatever he found in his reading, so that his work has more the appearance of a commonplace book, afterwards to be arranged by the author, than of any thing else ; there is everywhere a want of arrangement, a want of digestion into order. But it is very probable that Sauvages would have done a great part of what I have done, if time and leisure had allowed him. I have taken pains to distinguish his species by arranging them under the two heads of Idiopathic and Symptomatic. Wherever he finds a principal or very remarkable symptom of a disease, which likewise appears as a symptom in another disease, he has repeated it as a species in both places, and thereby multiplied the species, to the confusion of the student, to an unreasonable number. I have separated the idiopathic and symptomatic species of Sauvages, but I have further given to both quite a new arrangement. I have placed those together which I suspect to be one and the same, and next, those which have the nearest affinity to them. In many places, again, where Sauvages has multiplied the genera, and repeated the same species under each of them, I have taken care to put these together so that you may know the real number of species. The effect of all this, I think, must be to lessen considerably the whole number ; and it must be comfortable to the student to see that his object, which was seemingly so large, may be comprehended in a narrower view." (Works, vol. i. p. 464.)

In comparing Dr Cullen's definitions of the classes,

lar systematic precision in respect of genus and species. " It affords," adds Dr Good, " one of the most excellent specimens in the entire scheme of perspicuous description and accurate discrimination."

orders, genera, and species of diseases, with those of his predecessors, an impartial judge can scarcely fail to be struck by the superior practical knowledge of diseases evinced in the selection of those groups of morbid phenomena by which he has characterised them, and by the success with which, in making that selection, he has in general pursued a middle course between redundancy and obscure brevity. It is much to be regretted that those nosologists who, subsequent to Dr Cullen, have attempted to improve this branch of medicine, such as Drs Young, Good, and Hosack, should have bestowed so small a share of their labours on the important duty of improving the definitions of diseases, by pointing out such changes on those proposed by him or his predecessors, whether in the way of alterations, omissions, or additions, as a fuller consideration of individual diseases, or the more advanced state of medical science in general, might suggest; and should have devoted so much industry and learning to the comparatively trivial objects of methodical arrangement and nomenclature. Without wishing to derogate from the merits of such attempts as these authors have made to improve this department of Nosology, I feel authorized, by a careful review of their writings, to believe that the amount of their improvements, in respect of importance and number, is far from justifying the kind and degree of disparagement of this part of Dr Cullen's nosological labours, in which some of his critics, and particularly Dr Young, have indulged.

Dr Mason Good, who cannot be suspected of any undue partiality for Dr Cullen's nosological labours, has

had the candour to pay him the compliment justly due in regard to his definitions. "The definitions of the genera and species," he remarks, "were generally re-composed by Dr Cullen, and apparently modelled in consonance with the reformer's own practical observations." "The author shewed evidently that he had laboured his attempt in no ordinary degree ; and many of his definitions discovered a mastery that had never before been exemplified : pictures painted to the life, and of proper dimensions." "To this extent of praise," adds Dr Good, "Dr Cullen's system is fairly entitled ; an extent which ought ever to be borne in mind amidst the numerous, and, in many instances, exaggerated exposures of its defects which have lately been exhibited, and which it seems to be a growing fashion to detail both at home and abroad, more especially in Germany, where it has been asserted *ex cathedra*, and believed by extensive audiences, that, after all his pretensions, Cullen has done little or nothing for the improvement of nosology."

Whatever estimate may now be formed of the value of Dr Cullen's nosological labours, it cannot, with any degree of fairness, be alleged that his *pretensions* in regard to them were extravagant. On the contrary, he everywhere speaks, in his lectures and writings on this subject, of what he had done in this department with the utmost modesty and diffidence, endeavouring at all times to impress on his hearers and readers, that nosology, like the other branches of medical science, must necessarily be progressive in its advancement, and that it is only by frequent and multiplied trials that it can be brought to any degree of

perfection. In the preface to the second edition of his Synopsis, he remarks, that probably as long a period would intervene between the appearance of Sauvages' work, and a good system of nosology, as did between the first attempts at botanical arrangement by Cæsalpinus and the writings of Linnæus, adding that he himself would be contented with the humble praise to which Morison the botanist aspired, viz. the merit of exciting others to the study of his favourite science.

The second object of Nosology, the nomenclature of diseases, is one which has furnished much scope for the display of classical erudition. The names imposed upon diseases, in the earliest periods of medicine in Greece, were derived, as has been well shewn by Galen, in the second book of his treatise on the Method of Curing, from several different sources. Some of them were taken from the part affected, as peripneumonia, ischias, podagra, ophthalmia, dysenteria, &c.; some from the predominant symptom, as ileus, tenesmus, spasm, paralysis, dyspnœa, coma, &c.; some from these two circumstances combined, as cephalalgia, otalgia, cardialgia, odontalgia, hysteralgia; and some also from the supposed proximate cause, as melancholia, cholera, &c. To these may be added a considerable number of names of diseases derived from some imaginary resemblance to external objects, as elephas, cancer, polypus, anthrax, &c.; and after this, there still remains a considerable number, the origin of which it is not now easy to trace.

It is obvious from this mere enumeration of the various sources from which the Greek nomenclature of diseases was originally derived, that, even supposing



it to have been everywhere adopted, various temptations to amend or reform it could not fail to present themselves in the progress of medical knowledge. If the seat of a disease was supposed to be more accurately ascertained than at the time its name was imposed, or if the opinions of pathologists respecting the proximate cause of a disease came to be altered, medical men naturally became desirous to get rid of any erroneous opinion, on either of these points, that was involved in its designation. Where a name seemed to have no particular signification, they were tempted to replace it by one expressive of some important character of the disease. If it were found that two or more diseases had been comprehended by the early physicians under one common designation, then it became necessary, in separating them, to invent new names for one or for all of these distinct diseases ; or if, on the other hand, it was found expedient to comprehend two or more diseases which had been recognised as distinct by the ancient physicians, under one designation, then either an entirely new name was to be framed for this, or a choice was to be made among those which had previously been attached to the several species.

The idea of rendering medical nomenclature *uniform*, by deriving the names of diseases from one source only, or from a certain or fixed combination of sources, was also calculated to incite many to attempt its reformation. In recent times, two circumstances have tended very powerfully to encourage attempts of this kind ; *first*, the great and rapid progress that has been made in pathological anatomy, and the light which it is conceived to have thrown on the na-

ture and the seat of many diseases; and, *second*, the great success that has attended the introduction of an uniform system of nomenclature into chemical science.

It has been alleged, indeed, by Ploucquet, that “the name of each disease or species should be so characteristic and significant, that a person slightly acquainted with the language and the subject should, on hearing it, immediately understand what is the nature of the disease it designates.” It seems very obvious, however, that if the name of a disease ought to convey an accurate idea of that which it represents, it must be composed of the same elements as the definition of the disease,—in fact, it must be the definition converted into a denomination; consequently it must be derived either from the supposed proximate cause, or from the symptoms of the disease. But a term expressive of the nature and the seat of a disease, may very possibly be erroneous in respect of each of these facts singly, or of both together; and even in the case of inflammatory affections, in which the symptoms during life, and the morbid appearances met with after death, might be thought sufficiently characteristic to guard us against the risk of error, some recent attempts to replace old names of diseases by terms of more significant import, as in the localization of fevers, of tetanus, and of dysentery, prove that the practice is, in several respects, objectionable.\* And as to the

\* M. Bayle, in observing that the names of diseases ought not to be derived from anything which is variable in science, however incontestable it may appear, remarks,—“It is from not having followed this principle, that we see the name meningo-gastric fever succeed to that of bilious fever, that of adynamic fever to that of

proposal of comprehending, in the name of a disease, a summary of its elementary symptoms, a little reflection upon the number of these that usually enter into a definition will suffice to shew how hopeless an undertaking it would be to attempt to construct out of them any other kind of names than those *sesquipedalian* terms which all nosologists concur in condemning, at least when employed by others.

But whilst such have been the temptations to the substitution of new nosological designations for the names by which diseases have been recognised in former times, the inconveniences that have resulted from the practice have been by no means inconsiderable. Some of these inconveniences seem to have been experienced or anticipated at a very early period of medical science. The passion for changing the usual language of medicine called forth, on several occasions, the indignant rebuke of Galen, particularly in his treatise on Dyspnœa, in reference to the terms employed by some physicians of his own times to designate the simple diversities of difficult respiration; and in his treatise on the Differences of Pulses, he derides, with all the arrogant superiority of a Greek, the barbaric terms which Archigenes had proposed to apply to the eight qualities of the Pulse which he was disposed to recognise.

Sauvages, in his original sketch of his Nosological System, insists on the inexpediency of abandoning, except when it is unavoidable, the names of diseases

putrid fever; and that, unquestionably, these new names will, in due time, be replaced by others." *Considerations sur la Nosologie, &c.* Paris, 1802.

received and adopted by the ancients, or of substituting new ones in their place, without an extreme necessity ; for words, he remarks, are good only in respect of their signification. “ I have not chosen,” says he, “ to take upon myself to give new names to diseases ; and I have preferred the circumspection of Ray, Plumier, Winslow, Linnæus, and Artedi, to an exactness which might have been regarded as too bold.” At the same time he admits that it always has been, and always will be permitted to give new names to new diseases, and to select the best out of those which are in use. Accordingly, he specifies the considerations by which he had been himself influenced in his choice of the names of diseases ; as not the least important of which may be mentioned his remark that such names of genera and species as indicate the proximate cause, in place of the leading symptom, are liable to lead into error. “ If the employment of such names were admitted,” he observes, “ new names of diseases would be required whenever a new hypothesis is started. Though the doctrine of proximate causes be subject to changes, the nomenclature of diseases ought to be invariable.” These views on nosological nomenclature were afterwards more fully expounded by Sauvages in the *Prolegomena* to his *Methodical Nosology*. (§. 93, &c.)

Dr Cullen, also, was strongly impressed with the conviction that the nomenclature of diseases, confirmed by long use, ought not to be changed without very urgent reasons.

“ In the attempts I have made towards a method,” says he, “ I have nowhere introduced one single new name of a

disease, but have adopted names which had been employed by one or other of the systematics before me. I found it absolutely necessary, indeed, to make a choice, and upon many occasions I have not taken the name which was formerly the most common ; and in other instances, where I have adopted names, I am not quite satisfied that they are the most proper ; but rather than occasion any sort of confusion by new names, I have avoided every thing of that kind."

This reserve on the part of Sauvages and of Cullen has not been followed in all instances by other nosologists.\* Dr Cullen complains of Linnæus that he had frequently changed medical nomenclature without any urgent necessity ; and of Vogel that he had disfigured nosology by the introduction of new, frivolous, and inappropriate appellations. Dr Mason Good says of the nosological system of Ploucquet, that "it is singularly distinguished by the author's fondness for long crabbed words ;" and alleges that "Pinel has betrayed singular itch for changing established terms, which, in many cases, require no change whatever ; and superseding them by others which are neither more true to correct theory, nor more euphonous to a correct ear." Dr Mason Good himself, in professing the correction and simplification of nosological nomenclature to be one of the great objects of his system of nosology, has explained, with much erudition, the general rules by

\* "New terms," as has been observed by a statesman who was equally distinguished by his knowledge of literature and philosophy (the late Sir James Mackintosh), "are generally the easy resource of the unskilful or the indolent, and often a characteristic mark of writers who neither know nor love their own language." (Edin. Review, vol. xxvii. p. 192.)

which he has been guided. But however specious these may appear when considered in themselves, no one who has attempted to make use of that work for the purposes of study or of instruction can have failed to feel, in the multitude of names employed in it which are not used by any other authors, at least of modern date, a serious drawback on any advantages it may possess in the correctness of the principles on which its nomenclature is founded. Dr Good, however, seems to have been aware of the great inconvenience which results from incorporating particular pathological doctrines in the language and nomenclature of diseases, and he animadverts in severe terms on the following up the coinage of new systems by the coinage of new terms, as one source of imperfection and perplexity in Nosology. "This," says he, "has been a very ample and inveterate medium of error, and one which has not merely run through our nomenclature, but through our reasonings—inso-much, that through its prolific influence, the language of medicine has become a curious mosaic of the chief speculations of ancient and modern times." (Prelim. Diss. p. 1.)

It is not perhaps possible to assign a better reason for adhering to the names of diseases given to them by the ancients than has been done by the accurate and philosophical Baglivi. "The moderns," says he, "ought not to be opposed to the ancients, but, as far as is possible, to be joined with them in perpetual alliance. For what can be more absurd than when they agree in fact, to make them disagree in words. This passion for inventing new terms retards, in a wonderful degree, the progress of the tyro, and involves him in difficulty

and doubt. As respects patients, it is of little matter to them whether you designate the effects of disease, and the elements of the living body, by the less polished terms of the ancients, provided you have a perfect knowledge of the true principles of treatment, so that your words may pass into facts, and the events correspond with your predictions."

There is one circumstance which seems to have been very much overlooked by those who have occupied themselves with the reform of nosological nomenclature, viz. that many of those diseases which received generic appellations from the ancients, may proceed from different internal morbid conditions, as in the case of cynanche, of apoplexy, of ileus, of jaundice, &c., and that, to distinguish between these different forms of the same nosological disease, it is sufficient to annex a specific to the generic appellation, without inventing an entirely new name for each distinct form which we may be disposed to recognise. This principle was well understood by Sauvages, who remarks, "that to each genus there should be attached one name, and that as simple as possible; and each species of that genus should be designated, not by the generic name only, but also by an epithet, or specific name, added to it. The fewer the generic names or genera, the more easily are they retained and understood. But if as many genera as species be formed, then they are multiplied without necessity, and to the great injury of science, for medical nomenclature falls back to its original chaos, and all method is useless."

Besides giving a special definition of each particular disease, and assigning to each a peculiar appellation,

physicians have conceived it to be advantageous, with a view to diagnosis, to arrange the whole of the diseases which they recognise under more or less comprehensive groups, and the consideration of the different plans which may be pursued in such arrangements, forms, as we have seen, the third object of Nosology, viz. Classification.

It is obvious that any single character or combination of characters, in respect of which particular or special diseases agree with, or differ from, one another, such as their remote or their proximate causes, their seats, their symptoms, &c., may be made the basis for their methodic arrangement under a larger or smaller number of divisions, or, in the language of logicians, of higher and lower genera, and, in that of naturalists, of classes, orders, or families, and genera; or that, whilst one such character is employed as the ground of distinction between the divisions or classes, another character may be made use of for the same purpose in respect of the subdivisions or orders, and a third for the subordinate orders or genera. Accordingly, of the various plans upon which diseases have at different times been arranged under a larger or smaller number of divisions and subdivisions, some have had a relation to the remote causes from which they have been supposed to proceed,—others to the part of the organized frame, whether a texture, a system, or an organ, which they have been supposed to occupy,—others to the more obvious symptoms which they have exhibited in their progress, particularly as indicating the function more especially deranged,—others to the more



striking features of their general course,—and others, again, to the particular internal morbid conditions, structural or functional, on which they have been supposed to depend.

It would be absurd to imagine that an arrangement of diseases under higher and lower divisions, or classes, orders, and genera, executed upon any one of these principles, or on any combination of them, could be without its advantages to medical science. Diseases, like all other objects in nature, should be contemplated by those who profess to study them, from every possible point of view ; and every circumstance of resemblance or of difference, if duly investigated, cannot fail to suggest important considerations in the history of the particular species. It is impossible for the medical inquirer to prosecute his investigations into the seats of diseases, without aiming at an arrangement of them calculated to mark their relations to the textures, systems, and organs on the morbid conditions of which they depend. It is impossible for him to endeavour to arrive at a knowledge of the different internal morbid conditions which constitute the proximate causes of diseases, without being desirous to determine what particular or special diseases proceed from each of them. It is impossible for him to turn his attention to the different kinds of agents which, by their operation on the economy, induce its different morbid conditions, without endeavouring to refer to each of these—the remote causes of diseases, whether common or specific—the particular diseases to which it may give rise. But it is not to be expected that there will be found a strict coincidence in the arrange-

ments founded upon these different grounds of classification, nor can we reasonably hope to devise an arrangement equally applicable to all these several purposes ; and it should never be forgotten that an arrangement of objects is good or bad, not in itself, but as respects the purpose which it is intended to serve.

The principal, if not the sole purpose for which it has been deemed expedient to establish systematic or methodic classifications of the objects of natural history, of minerals, plants, and animals, is to facilitate the reference of individual specimens to their particular species. It is obvious, therefore, that the classification which tends in the greatest degree to facilitate this reference, must, so far as this particular object of the natural historian, the distinction of species, is concerned, be the best. But classifications of this kind are not inconsistent with, or contradictory of, arrangements proceeding upon other principles, and intended to serve other purposes. That the botanist finds it convenient to classify plants according to the methods of Linnæus or Jussieu, founded on their correspondence in some common external characters, does not prove the gardener to be in error because he arranges them according to the climate or to the soil bestsuited to their growth, to the mode of their perpetuation, to the dimensions which they attain, to their adaptation to purposes of ornament or of use, &c. &c. ; nor does it prove the physician to be wrong, because he arranges plants according to the nutritious, medicinal, or poisonous powers of the principles which they may contain.

So also in regard to diseases. If it be considered

desirable to frame classifications of these, with a view to assist the medical practitioner in referring individual cases to the particular species to which they belong, then the classification which facilitates diagnosis in the greatest degree, must, as regards this purpose, be the best. But it by no means follows that, for other purposes, other arrangements may not be proper or may not be required.

In natural history, it is esteemed of importance that the characters on which the genera, orders, and classes are founded should, like those employed for marking the species, be of easy recognition; and there can be no doubt that the same principle ought to be kept in view in a nosological classification intended to assist diagnosis. A slight comparison of the characters on which, as we have seen, it has been proposed to establish the higher divisions and subdivisions of diseases, seems sufficient to shew that it is the symptoms which they exhibit that constitute the marks of correspondence and difference most easy of recognition; and hence, whatever advantage it may be found possible to take of correspondence between symptoms and other characters, as remote cause, seat, pathological condition, &c. in framing nosological classes, orders, and genera, it is the symptoms that should constitute the groundwork of this portion of methodic arrangement, as well as of the distinction of particular diseases. It may, indeed, as we shall afterwards have occasion more particularly to notice, be a matter of question, how far the framing of classes and orders of diseases is requisite for the purpose of diagnosis; but if a classi-

fication under higher and lower genera, having for its object to facilitate the reference of individual cases of disease to their particular species, be considered desirable, that object ought to be strictly held in view in the execution of the project.

It has been usual, indeed, for nosologists, imitating the language of naturalists, to speak of nosological arrangements of diseases as being more or less artificial or natural. By an artificial arrangement seems to be meant one in which the points of resemblance or difference between the objects brought into close approximation in the several orders and classes, have relation to obscure or trivial characters; and by a natural arrangement, one that brings together objects which correspond in their most obvious and important relations, phenomena, or characters.

The classifications adopted in the several departments of natural history, shew that, for purposes of distinction, it is of little or no consequence how trivial the character or characters employed to mark the classes and orders may be, provided they are fixed, steady, and determinate in the members included in the subdivisions, and that they are readily recognisable; or, in other words, that an artificial arrangement of these objects is, in this respect, as advantageous as a natural one. And in the same way, in a classification of diseases intended to be subservient to diagnosis, every circumstance of correspondence or difference, however trivial it may appear in itself, may be taken advantage of in constituting the higher and lower genera of diseases, which will assist us in the easy reference of individual cases successively

to the class, the order, the genus, and ultimately the species to which it belongs.

Many of the objections which have been urged against particular nosological classifications, as bringing into close approximation diseases which, in their intimate nature, are widely different from one another, or as being “artificial” or “not natural,” seem to have originated in inattention to the sole purpose which such classifications are intended to fulfil, that, namely, of facilitating diagnosis; and it may, perhaps, be added, that, in many attempts at nosological classification, a similar inattention to its proper object has been betrayed.

When nosologists have spoken of a natural arrangement or classification of diseases, they seem generally to have understood one founded upon the various internal pathological conditions which give rise to special diseases, such as inflammation, hæmorrhage, pain, spasm, &c. For though the several diseases depending on any one of these proximate causes differ in many of their phenomena, according to the part of the body in which they are seated, yet still their community of cause occasions a considerable affinity of character. A methodical arrangement of this kind, or on this principle, has been considered the more desirable, as being likely to bring together diseases, corresponding not only in some very important relations as regards their symptoms, but also in the indications and means of treatment which they suggest and require. If we are to attempt to establish a general doctrine of inflammation, of hæmorrhage, of spasm, &c., as so many morbid conditions which, occurring in

different parts of the body, constitute the proximate causes of special diseases, then there can be no doubt that, to enable us to ascertain the general history of any of these morbid conditions, we ought to bring together, under separate heads, the various special diseases which we believe to appertain to each, the inflammations, the hæmorrhages, the spasms, &c. This is obviously a purpose which the arrangement of diseases under natural families is well suited to promote. But it does not necessarily follow that the same arrangement will be found advantageous for purposes of diagnosis. An arrangement of this kind must vary according to the progress of knowledge and of opinion, for a disease which is at present supposed to depend on one pathological condition, may be found to proceed from another. It must separate diseases which, though depending on different pathological conditions, bear a striking resemblance to one another in their external phenomena, as the different species of apoplexy, of epilepsy, and of jaundice; and it may bring together diseases which, though belonging to the same natural family, may respectively be characterised by groups of symptoms which do not bear any very obvious resemblance. Thus, in the hæmorrhages, diagnosis would be little assisted by the bringing together of apoplexy and hæmoptysis.

Dr Cullen, following the example of Sauvages, deduced the characters of his classes and orders, as well as those of his genera and species of diseases, from external phenomena or symptoms—the definition of each higher division containing an enumeration of characters common to all the lower divisions

comprehended under it. Several more recent nosologists have not considered the defining of classes and orders *symptomatically*, to be essential to nosological classification. They seem disposed to believe that, as the advantages which naturalists derive from defining their classes and orders by appreciable characters, depend on the immense number of objects between which their comparison must be made, the physician, from the limited number of special diseases, may dispense with this assistance; and that an arrangement founded on some other principle may prove more advantageous to the teacher or student of medicine, particularly an arrangement which shall bring diseases into approximation with one another, according to the part of the body principally affected, and the function principally disturbed. On this principle Drs Young and Good, in establishing their classes and orders of diseases, in imitation of Ploucquet, on an Anatomico-physiological basis, have not defined but merely interpreted or paraphrased the terms employed to designate them; and, indeed, it may be doubted whether, if the classes and orders of diseases be founded on anatomico-physiological considerations, we can expect to be able to assign to the groups of diseases thus formed, symptomatic definitions in the manner pursued by Sauvages and Cullen, seeing that it is by no means easy to mark common characters in which the subdivisions shall correspond. But even with this disadvantage, it must be allowed that an anatomico-physiological arrangement of diseases is the one best adapted for lectures or for treatises on the Practice of Physic, as bringing together, in the first place, the

different diseases of the same organ, and, in the second place, those of the organs most intimately related to one another. That it may be pursued with advantage, however, it is requisite that the student should have been previously instructed in the general doctrines of disease. Under each division must be included special diseases that depend respectively on inflammation, hæmorrhage, spasm, &c. and a previous knowledge of these pathological states is consequently indispensable. In teaching upon an arrangement founded on natural families, there may be the opportunity of discussing the general doctrines as an introduction to the particular family; but where the arrangement is anatomico-physiological, no such opportunity can present itself.

It does not appear that any divisions or arrangements of diseases adopted by the ancients had the promotion of diagnosis for their particular object. They were merely divisions, for the most part under two heads, dichotomous or correlative, founded on some striking contrast in the history of particular diseases; as, for example, that into internal and external, or into acute and chronic diseases, into those common to both sexes or to all ages, and those peculiar to the female sex or to infancy, &c. They were, in short, what Sauvages has termed synoptical, in contradistinction to systematic arrangements; and, perhaps, served little purpose beyond that of affording a ground, as Hildenbrand remarks, for subdivisions of treatises on medicine into books, chapters, sections, and paragraphs.

Galen, in his "Treatise on the Method of Curing,



addressed to Glauco," mentions, in terms of high commendation, an Athenian physician, Mnesitheus, as thinking that "beginning by the first and highest genera, these should be again divided into other genera, species, and differences; these, in like manner, into others, which again should be distributed in the same manner, till we descend to some such species as will admit of no subdivision." And Galen follows up this reference by expressing his own opinion, that whatever faults are committed by other sects and by many physicians in the cures of diseases, a faulty division is their first and chief cause. "For some," says he, "stop short at the first and highest genera, contenting themselves with those indications only which are taken from them; others make a certain progress in division, but do not arrive at the end, and many divide erroneously."

This passage has been repeatedly referred to as proving that Mnesitheus had proposed a classification of diseases, of which Galen approved; and Sprengel even refers (*Hist. de Med.* vol. i. p. 375) to Galen as saying that Mnesitheus rendered himself very celebrated by the classification which he established among diseases. It by no means appears, however, that the passage refers particularly to the division of *diseases*; and at all events, it cannot be construed into any thing more than the approbation of a general principle in logic, that, namely, of systematic division.

Galen himself, in his treatise on the Differences of Diseases—the threefold object of which he professes to be, to determine 1st, what disease is; 2d, how many there are of universal, simple, and primary diseases,

the elements, as it were, of others; and, 3d, how many diseases are compounded of these,—has sketched what may perhaps be regarded as the outlines of an arrangement of diseases, under these two heads, of Simple or Primary and Compound. He establishes his differences of diseases on the division of the parts of the body into similar and organic, or, in the language of the moderns, those of textures and organs, and endeavours to accommodate it to either the methodic doctrine of dilatation and constriction of the passages, or to the Democritic doctrine of the four elements. Galen has not, however, attempted an enumeration of the diseases referable to each of the divisions of which he has thus sketched the plan.

The Arabian physicians and their successors, as Sennertus has remarked, passing over the general methods of arrangement pursued by the Greeks, considered diseases, with their causes, signs, and cures, in the order of the parts of the body in which they occur, as those of the head, of the chest, of the abdomen, &c.; and this arrangement Sennertus, in designating it the customary order of practical physicians, himself adopted in his *Medicina Practica*, published in successive parts between 1628 and 1635.

To Felix Plater is attributed the merit of having first proposed, in his *Medicina Practica*, published in 1602, an arrangement of diseases into classes, founded on the more remarkable analogies of their phenomena or symptoms, or, in his own words, “according to those manifest affections which fall under the senses, or which are perceived by the senses of the sick, or of the bystanders, and concerning which patients first complain

to their physicians." The highest genera or first heads established by Plater, in conformity with this view, were, 1st, The lesions of the functions, comprehending those of Sense and Motion ; 2d, Pains or Uneasy Sensations ; and 3d, Those morbid affections which have been termed Vitia, and which may be considered as corresponding with structural alterations.

But, as has already been observed in speaking of the definitions of diseases, it is to Sauvages that medicine is indebted for the first system of Nosological Classification, executed in conformity with those logical principles which had been applied to the classification of the several subjects of natural history ; a classification having for its purpose, by a succession of divisions and subdivisions, to each of which is attached a definition expressive of certain determinate and obvious characters common to all the lower genera comprehended under it, to enable the practitioner gradually to circumscribe the number of objects of comparison, and thus arrive at length at the precise species to which the particular individual case under his observation is referable.

The principal advantage calculated to result from the institution of nosological classes and orders, appeared to Dr Cullen to be the necessity which every such attempt imposes on those who engage in it, of marking very accurately the characteristic phenomena of particular diseases.

"In natural history," says he, "it is the attempts towards a system that have produced the knowledge of particulars ; and though these are, in their turn, necessary to render the system perfect, it has, however, been especially every new ef-

fort in system that has excited a new industry in completing and rendering more accurate the knowledge of particulars. The system of Tournefort, for example, led to a more exact observation of the form of flowers; that of Rivinus directed attention to the number and disposition of the leaves; a slight attempt by Vaillant and Boerhaave led to the studying of stamens and pistils: and the system of Linnæus has carried the same much farther. In short, I think every body acquainted with the progress of natural history must know that the attempts in system and the study of particulars have mutually promoted and supported each other.”—“It is certainly the same with regard to diseases. If a system—a nosologia methodica—cannot just now be rendered tolerably perfect, it is a certain proof that the particulars of which it should be formed are at present neither accurate nor complete; and it is equally probable that they cannot be rendered so till our attempts in system have been repeated, and have made some farther progress. Our attempts in system are necessary to enlarge our stock of facts. The formation even of classes and orders may, in many cases, contribute to a fuller and more exact distinction of the species. For though we cannot at all times obtain a certain and accurate arrangement of this kind, yet the very attempts to obtain it must be of great use, by leading every now and then to useful discussions both in pathology and in the history of diseases. Diseases must be distinguished both by the symptoms peculiar to each, and by those common to each with some others; which is nothing else than to distinguish diseases as all other things in nature should be distinguished,—by species and genera, and a distinction of this kind necessarily requires the noting both of the lower and of the higher genera.”

Dr Cullen never set a high value either on his own labours or on those of other nosologists, so far as these

had merely for their object the arrangement or classification of diseases.

“ Those who have applied to this study before us,” he remarks, “ have, as it appears to me, gone improperly about the matter ; for, paying little attention to the species, they have been occupied in constituting classes, orders, and genera. But by nature, species only are given : the formation of genera is the production of the human mind, and must be fallacious and uncertain till all the species are well known and distinguished ; and our labour in constituting genera, unless we always have regard to the species, will be vain and futile.”—“ I am so far from thinking that we ought to enter anxiously into the general systematic arrangement, that, in my *Prolegomena*, I have insinuated a contrary notion. Because I think there is use in the attempt, I have, with others, endeavoured to form classes and orders ; but for the accuracy of these, I would by no means vouch. I have many difficulties still with regard to them, and I think I could still point out considerable improvements ; but my labour is to study genera in the first place, and, when I can arrive at them more certainly, species. If I can distinguish these with tolerable accuracy, I am not concerned with my distribution into classes and orders being more or less exact. It is from our accuracy with respect to the species that all the rest of the divisions can be properly established ; and in nosology, indeed, we are very much relieved, by the smallness of the number of the objects to be arranged, from a more particular attention to classes and orders.”

“ When the things to be distinguished are very numerous, it seems to be very useful and necessary, both for the investigation and remembrance of them, that they should be referred to some higher genera ; but when the species of these things are not very numerous, and it does not exceed the powers of most men’s memories to remember them, there

scarcely seems any necessity of referring them, with great solicitude, to classes and orders."

With Dr Cullen's recorded statement of the slight degree of importance which he himself attached to the portion of his nosological labours relating to systematic arrangement, it seems superfluous to enter on any lengthened consideration of the various criticisms to which this part of his work has been subjected; the more so that a large share of these criticisms were anticipated and discussed by himself, either in the Prolegomena or in the Notes to his Nosological System, though his remarks respecting them have seldom, if ever, been recognised or adverted to by his critics, who have brought forth their objections as if they were entirely original, and as if they afforded so many proofs of their own superior discrimination and knowledge of the subject. A few remarks, however, on some of the more prominent of these criticisms will best serve to shew how far they should be considered as detracting from the general merit of Dr Cullen's Nosological System.

Dr Cullen, as is well known, arranged diseases in that system, under four primary divisions or classes, viz. Pyrexixæ, Neuroses, Cachexiæ, and Locales. Dr Mason Good has objected to this division, on the ground that "the class Locales has no scientific relation to the other three classes, no parallel or apposition with them. To have brought it into any such kind of bearing, the whole of the three first classes should," he alleges, "have been denominated conjunctively Universales, as has been done by Dr Macbride. But this," he adds, "would have destroyed the general casting of the ar-

rangement, and have produced a division which was not wanted, and perhaps does not exist." (P. 16.) But Dr Good is mistaken in supposing that Dr Cullen had not in view, in framing his nosological system, the general division of diseases into the Universal and the Local, the division long ago followed by Celsus in his very elegant treatise on Medicine ; for Dr Cullen was himself accustomed to make the following remark in his lectures, in reference to his nosological distribution.

"From the term applied to the last class, you must perceive that I mean to make a general division of diseases into Universal and Local, or into those which affect the whole system at once and those which affect one part of it only. Here there are some difficulties, but it is enough to give what is very general." "It may not," he elsewhere observes, "be at all times easy to distinguish between the diseases of the whole system which are included under the three first classes, and those of a single part, or the local diseases that are placed in the fourth class. We acknowledge that this may sometimes happen, though rarely. But nothing better at present occurs to us, and, for the reason stated in the Prolegomena, we are not very solicitous about a perfect arrangement of classes."

It may be doubted, too, whether the rules and practice of natural historians, in framing their classes, imposed any obligation on Dr Cullen, formally to express the contrast which he intended to make between the three first and the fourth of his classes. Linnæus institutes no such contrast between his twenty-fourth class of plants, the Cryptogamia, and the twenty-three preceding classes ; and it might perhaps as reasonably be alleged that Dr Cullen, in styling his first class *Pyrexiaë*, should, in order to bring it into bearing with

the other three classes, have comprehended them under a common title of *Apyrexiaë*.

In his division of universal diseases, Dr Cullen endeavoured to combine the advantages of a symptomatic method with one founded upon physiology. "The idea of my *Nosology*," he remarks, "proceeds upon the view that pathology generally considers symptoms as the effects of morbid derangements of the three several classes of functions, the vital, animal, and natural. Our *Pyrexiaë* comprehend the vital functions; the *Neuroses*, though extending to the whole of the functions, have chiefly in view the animal; and the *Cachexiaë* comprehend the affections of the natural functions." This plan of arranging diseases according to the functions which they principally affect, was still farther extended and improved by Professor Ploucquet of Tubingen, who established seven classes, comprehending the diseases of the Nervous, the Circulatory, the Respiratory, the Digestive, the Secretory and Excretory, and the Reproductive Functions, and those in which there occur Changes of the Sensible Qualities. Ploucquet's arrangement was pursued, with only slight variations, by Drs Young and Good in their nosological systems, the former reducing the number of classes to five, viz. the Nervous, Sanguine, Secretory, and Structural Diseases, and Displacements; and the latter, again extending them to seven, viz. the diseases of the Digestive, Respiratory, Sanguineous, Nervous, Sexual, and Excrement functions, and Fortuitous Lesions or Deformities. Notwithstanding the great diversity in the Greek appellations bestowed by these nosologists on their several classes,



it is easy to see that they have all proceeded on the same anatomico-physiological principle in framing them, and that, in fact, they are for the most part the same divisions under different denominations.

Conceiving that the diseases which systematic authors have been accustomed to designate as Febrile, exhibit such a number of phenomena in common as to admit of their being advantageously comprehended in one nosological class, Dr Cullen applied to them the general term of *Pyrexiaë* ; and, in his definition of this term, endeavoured to mark the external phenomena, their agreement in which has led to the grouping of diseases, in other respects so different, under a common title. Fully sensible of the difficulty of this attempt, he has himself, in different parts of his writings, taken occasion to point out the imperfections with which his own definition is chargeable, and has thus amply and ingenuously anticipated and recognized most of the objections that have since been urged against it,—objections which, judging from the trials hitherto made, it would seem to be much easier to repeat than to obviate.

“ I may here,” says he, “ be allowed to offer an excuse for what may perhaps be censured, both in this and in several other instances. Thus it may be said that febrile diseases are sometimes seen which had not been preceded by any shivering, or in which there is neither greater frequency of pulse nor greater heat, than is usual in health ; and that, therefore, the character given of this class of diseases is neither true nor universally applicable. I will not deny that febrile affections of this kind, though very rarely, are sometimes seen ; but in characters of classes to be employed for the purpose of distinguishing species, it does not appear ne-

cessary that the whole of the marks of the class should be present in every species, and it is sufficient if most of them be present in each species."

And after applying to the definition of this class of diseases, the general rule that "every proper character should point out a concurrence of several marks or symptoms," he observes :

"We have as far as possible attended to these considerations in the character which we have here given of Pyrexiaë ; but, in the characters of other classes and orders, I could scarcely expect to be so fortunate ; and, with respect to most of them, if the character can be properly applied to most of the species, I do not concern myself about a few exceptions ; it is enough for me to be in general useful ; I cannot expect to be always perfect."

Assuming, then, the phenomena enumerated in his character of Pyrexiaë, to be common to all the orders of febrile diseases, Dr Cullen conceived that *Fevers*, more strictly so called, which he established as his first order, are particularly distinguished by two circumstances, 1st, the pyrexial symptoms being preceded by languor, lassitude, and other signs of debility ; and 2d, their occurring independently of any primary local disease. Dr Southwood Smith, in a late criticism on Dr Cullen's definition of fevers, has jumbled together at random the definitions of the class Pyrexiaë and of the order Fevers, as if the several parts of these definitions had not been intended by their author to have any fixed relation with one another, and has then proceeded to make some very severe animadversions upon this supposititious definition. After stating that Boerhaave reduced the catalogue of symptoms which appear in all

fevers to three, viz. horror or shivering, frequent pulse, and heat, Dr Smith remarks :

“ To the catalogue of Boerhaave, Cullen makes the following additions : languor, lassitude, and other signs of debility, together with derangement of the functions, particularly a want of vigour in the limbs, without any primary local affection. This extension of the catalogue,” continues Dr Smith, “ adds in no respect to the excellence of the generalization. It has all the vices which a definition can possess ; the characters are not present in all cases ; the very opposite are strikingly prominent in many, while the last, ‘ without any primary local affection,’ has so direct a tendency to mislead the mind, and positively to prevent it from observing the real phenomena of the disease, that it may well be questioned whether the introduction of this single phrase into the definition of fever, has not been the occasion of far more practical mischief, than has been compensated by any good that has been accomplished, or ever can be accomplished, by all the rest of the nosology.”

With the vices of Dr Smith’s supposititious definition, Dr Cullen certainly is nowise chargeable. Of the defects of his own he was fully aware.

“ As some of the other marks of debility,” said he in his lectures, “ we might here add anorexia, nausea, and vomiting ; and further, that the pulse will be found to be always weaker before the attack. Here let me observe that these symptoms of debility are particularly to be found in the beginning of all intermittents, and in the most part of those continued fevers which we would arrange under typhus or synochus ; but with respect to the synocha, we have not an opportunity of observing the debility that precedes. The character of a disease, however, never rests on a single symptom, but on a concourse of them, so that whatever difficulty there may be in applying the symptoms of debility to syno-

cha, we are relieved by what is subjoined, ' without primary local disease.' "

The practical question at issue between Dr Cullen and Dr Smith, therefore, is, whether, in the ordinary course of fevers, the succession of symptoms is of the following nature, viz. languor, lassitude, and other marks of debility, succeeded or attended by a shivering, and this followed by increased frequency of pulse, increased heat, disturbance of several of the functions, and particularly diminished strength of the limbs, and all these occurring without there necessarily existing any evident morbid alteration of a particular texture or organ. That this general statement of the order of succession in which the phenomena of fevers manifest themselves, considered as a standard by which the practitioner may determine whether a particular case under his observation does or does not belong to the order Fevers, is not capable of improvement, I by no means undertake to affirm ; but that it has as yet received any considerable amendment, notwithstanding the host of criticisms to which it has been subjected, I feel very much inclined to doubt.

Dr Smith's objection to that part of Dr Cullen's definition of the fevers more strictly so called, which points out their independence of a primary local disease, is by no means very intelligible. It may be questioned as a matter of fact, whether any such pyrexial diseases actually occur ; or if this negative character does not establish such a difference between the fevers and other pyrexiaë, as the practitioner, on careful examination, is able to recognise, it may be improper to introduce it into the definition, even admitting it to be a

correct distinction. But unless it be assumed that no such diseases as idiopathic or *essential* fevers, as they are termed, exist, it seems impossible to conceive how the statement in the definition of fevers, of their *essentiality*, as a matter of fact, capable of being determined by observation, should have any tendency to mislead the mind, or to prevent it from observing the real phenomena of febrile diseases. Indeed, as Dr Cullen has himself stated, the want of any primary local disease might have been considered as in itself sufficient, taken in connection with the definition of the class, to characterise his order of fevers.

“Here,” says he, “you will take notice of the phrase Primary Local Disease, for if every word be not of importance, it ought not to be in the definition. The local disease,” Dr Cullen proceeds, as if in anticipation of some hypercriticism upon the expression, “may be very universal ; but here I use the term in the common acceptation. Thus, an inflammation may supervene, or, from some concurrent cause, may attend a fever ; but then, it is not the primary disease upon which the fever depends, so that the import of the whole definition is what constitutes fever.”

Dr Cullen was well aware that cases sometimes occur, in which it is difficult to determine whether the primary affection be simply a fever or a disease of one of the other orders of the Pyrexiaë ; and in his commentary on the seventy-third paragraph of his First Lines, he was accustomed to point out the considerations by which this matter may be determined. They must, therefore, it is conceived, entertain very erroneous notions of the purposes for which Nosology has been intended, or be very ignorant of Dr Cullen’s

writings upon fevers, who can concur in the strange and groundless opinion expressed by Dr Smith, that his introduction of the phrase 'without primary local disease,' into the definition of fevers, has produced more practical mischief than all the rest of nosology can compensate for.

In subdividing his order Fevers under sections, Dr Cullen framed his definition of Intermittent fevers so as that it should comprehend the Remittents, of which previous nosologists had formed a separate order. This arrangement, as he himself informs us, he adopted, from the considerations, that the fevers called remittent arise from the same morbid cause as intermittents, namely marsh miasma ; that they prevail together, epidemically, in the same places and at the same season of the year ; that they are cured by precisely the same remedies ; and that very frequently, in the same person, what seems to be the same disease exhibits at one time the type of an intermittent, and at another time that of a remittent fever. Diseases, therefore, so closely resembling one another in their causes, cure, and type, ought not, he conceived, to be referred either to different orders or to different sections. It must be acknowledged that the circumstances of correspondence between intermittent and remittent fevers which Dr Cullen enumerates, are calculated to establish a striking analogy between these two forms of febrile diseases, if not to prove their specific identity ; but all of them seem, both severally and collectively, to have been unaccountably overlooked by most, if not all, of those critics who have taken it upon themselves to censure this part of Dr Cullen's nosolo-

gical arrangement. (See Works, i. 246, 487, and 516.)

Another singular allegation of Dr Smith's, is that "the single fact suggested to the mind of the practitioner by the classification of true or idiopathic fevers under the two great divisions of Intermittent and Continued, is in the highest degree trivial." Whether the *single fact* of the *type* of fever, be or be not trivial in itself, every practitioner knows that it is a fact of the utmost importance in the inferences that are deducible from it ; seeing that it serves, in a great measure, to mark both the cause from which the disease proceeds and the particular or specific treatment which it may require.

Dr Smith also alleges, that, "of the particular groups of symptoms which have been brought together under the great class Continued fever, it is impossible to discover any kind of principle which has led to the formation of the distinct assemblages that have been made, or to their nomenclature when thus collected. Synocha, Typhus, Synochus, are the three genera," proceeds Dr Smith, "which modern nosology, in the power and pride of its strength, has put forth, as at once distinctive and exhaustive of this class of disease. The aggregate phenomena constituting Synocha form just that particular series which is common to some forms of fever and to all acute inflammations. The train of symptoms thus brought together do not alone form any variety of fever. The second group of symptoms forming Typhus, and the third forming Synochus, independently of their being brought together and named according to no known or even assigned principle, are liable to the farther and fatal objection, that they do not even occur in nature." (P. 62-3.)

In reference to these criticisms, it may be remarked

that, in endeavouring to establish distinct generic, or rather specific, forms of continued fever, Dr Cullen set aside those divisions which previous nosologists had deduced from the *duration* of the disease, regarding this as a character neither determinate in itself nor capable of furnishing to the practitioner the means of diagnosis ; and in place of these divisions, he substituted one founded upon the predominant symptoms of the disease, or on those symptoms by the presence or absence of which the method of treatment to be pursued is, in a great measure, to be determined. These symptoms appeared to him to be referable to two heads ; *first*, those which give evidence of increased action or inflammatory irritation in the system ; and *second*, those which give evidence of a weaker reaction.

“ This distinction,” says he, “ is the same with that of fevers into Inflammatory and Nervous, the distinction at present most generally received in Britain. To the first, as a genus, I have given the name of Synocha ; to the second, that of Typhus ; and, little studious whether these names be authorized by the ancient use of the same terms, I depend upon their being understood by the characters annexed to them in our Nosology, which I apprehend to be founded on observation.”

Dr Cullen was well aware, however, that the phenomena of Inflammatory fever, or Synocha, seldom are to be met with pure, but are generally accompanied with more or less of catarrh, rheumatism, or some other local inflammatory affection. By the term Typhus, Dr Cullen obviously intended to designate the most malignant form of continued fever, the putrid or slow nervous, the petechial, jail or hospital fever of different authors, which often runs its course without ex-



hibiting any distinct evidence of increased action or inflammatory irritation in the system. But whilst he thought that Synocha and Typhus, as representing respectively the predominance of inflammatory irritation and weaker reaction, might be considered as the fundamental differences of continued fever, and rejected those distinctions that had been founded on biliousness and putrescency, he acknowledged that the continued fevers of this country exhibit most commonly, not the one or the other of these forms separately, but a combination of them ; the phenomena of fevers being, as he says, greatly diversified and variously combined, and the symptoms of inflammatory and nervous fevers being often intermixed with one another. To fevers exhibiting such a combination and succession of symptoms, commencing usually as inflammatory and terminating as nervous, he proposed to apply the name Synochus. "My aversion to change terms," says he, "has made me employ this ancient word, which implies simply a continued fever."

In respect of the distinctions which it is desirable to recognise among continued fevers, it may be remarked, that when we consider the great diversity, in respect of the predominant symptoms, and particularly of those indicative—to use Dr Cullen's language—of inflammatory irritation or of weaker reaction, which may present itself in different individuals in the course of the same epidemic fever,—and that, even, an epidemic depending upon a specific contagion,—it seems very doubtful whether differences in the symptoms will warrant us in establishing more than one species of continued fever. And this doubt is strengthened when

we consider that in each of the febrile eruptions, depending on a single specific contagion, the character of the attendant pyrexial symptoms is liable to the same diversities which occur in the case of simple fevers. The circumstance of some continued fevers, sporadic or epidemic, seeming to arise from contagion, whilst in others no evidence of contagion can be detected, seems to present another ground of distinction between these fevers, to which, perhaps, Dr Cullen paid too little attention, as he seems to have considered Synocha to be the only form of continued fever which occurs independently of contagion. But here again, if contagious and non-contagious fevers are found to exhibit the same external characters, and if no differences can be detected in the morbid appearances presented by the bodies of those who sink under them, it may be doubted whether the mode in which continued fevers are produced will furnish a sufficient ground for specific distinctions between them.

Dr Good mentions as a palpable instance of Dr Cullen's having frequently found himself compelled, from the paucity of his classes, to overstep the natural boundary implied by the terms he employed to designate those classes which he established, the tribe Hæmorrhages (the third order of the class Pyrexixæ), which, he says, have no direct catenation with any idea suggested by Pyrexia in the common use of the term; and he farther complains that all the hæmorrhagic diseases are not brought together by Dr Cullen, some being included by him under the class Locales. Both of these objections have been taken notice of by Dr Cullen himself, in a note upon the order Hæmorrhages, in which, after ex-

plaining the grounds of his disinclination to admit into his Nosology such a class as the Fluxes of Sauvages and Sagar,—the Profluvia of Vogel, and the Evacuatory diseases of Linnæus,—and the necessity of finding places for the diseases which these nosologists had included under this class, thus variously designated, he makes the following statement :

“ Those hæmorrhages which are always accompanied by a certain degree of pyrexia, ought certainly to be placed under the class Pyrexiaë, and here, therefore, I have placed what are called the Active Hæmorrhages. I follow Hoffmann, who has treated of the hæmorrhages of this kind immediately after fevers, and has stated the reason in his preface, namely, that, when the phenomena are well considered, and their explanations are examined with some attention, it appears that eruptions of blood are produced by nearly the same internal movements from which fevers arise, though not sufficiently completed to produce fevers.”

It may be remarked, that Dr Good's objection to the recognition of hæmorrhages as an order of the class Pyrexiaë, would apply with equal force to the two other orders of the same class, Exanthemata and Profluvia, each of which has also a corresponding apyrexial order.

In reference to his attempts to decide what genera or species should be comprehended under the order of Exanthemata, Dr Cullen observed in his lectures—“ I find many difficulties in Nosology, and nowhere greater than in this place. I am truly not satisfied with this order. This may be considered as an objection to nosology in general ; but it merely shews that the history of diseases, as it now stands, is far from being complete and accurate ; and I say that it is the attempt at no-

sology which chiefly serves to point out these doubts, to start questions, and to direct our farther observations. Without it, the matter would have remained in the uncertainty, confusion, and obscurity, in which it has continued for ages past.” In no branch of practical medicine has the truth of these remarks been more fully confirmed since Dr Cullen’s time, than in that which relates to the nosological distinctions of the diseases of the skin, of which eruptive fevers form so important a part. The writings of Plenck, Alibert, Willan, and Bateman, and of their followers both in this country and on the Continent of Europe, have given a precision and interest to the diagnostic characters of these diseases, which the study of Nosology could alone have procured for them ; and, at the same time, hold out a strong encouragement to reiterated attempts at improvement in the diagnosis of other natural families of diseases.

Whilst Dr Cullen’s views respecting the agency of the Nervous System in the animal economy led him to believe that almost all morbid movements in this economy depend on movements in the nervous system, and that, consequently, almost all diseases might in some sort be termed nervous, he conceived that this denomination might advantageously be limited to those diseases which affect the nervous system almost singly, or at least primarily, without at the same time affecting the circulation of the blood, or the nature of the humours, unless in a secondary manner. Such diseases, which he conceived are to be found in all the classes of functions, it was his object to comprehend under his second class, Neuroses. In the *animal func-*

tions they present themselves under the character of diminished voluntary motions, with sopor or suspension of the senses (*Comata*); in the *vital* and *natural* functions, under the character of diminished involuntary motions (*Adynamiaë*); in all the motory apparatus of the economy, muscles and muscular fibres, subservient to the *animal*, the *vital*, and the *natural* functions, under the character of irregular motions (*Spasms*); and, lastly, in the functions exclusively *mental*, under the character of impaired operation of the judging mind, without pyrexia or coma (*Vesaniaë*).

An examination of the subdivisions of this class, into orders, genera, and species, as established by Dr Cullen, cannot fail, it is conceived, to satisfy the candid critic, that whilst all those affections which involve sensation, voluntary motion, and mental exercise, or in other words, the affections of the animal functions, are most properly and advantageously brought together under this head, the diseases of the several vital and natural functions which he has here introduced do not so obviously correspond with the definition of the class as to render their comprehension under it advantageous; and that an arrangement bringing them into nearer proximity with the other morbid affections of their respective functions, would be greatly more serviceable for purposes of diagnosis.

Dr Cullen's third class, *Cachexiæ*, which he characterised as depraved habit of the whole or a great part of the body, without primary pyrexia or neurosis, and which he divided under the three orders of emaciations of the whole body (*Marcores*), external swellings of the whole or a great part of the body, whether from

fat, wind, water, or solid growth, &c. (Intumescentiæ), and the cachexiæ principally deforming the skin and external surface (Impetigines),—has been the subject of various criticisms. He was by no means unaware of the difficulties to which his attempt to bring together, under one class and a common name, such of the general diseases of the natural functions as had not found a place in the classes Pyrexiaë and Neuroses, was exposed. In the brief introduction to the Cachexiæ, in his First Lines of the Practice of Physic, he thus expresses himself.

“ The term Cachexy has been employed by Linnæus and Vogel, as it had been formerly by other authors, for the name of a particular disease; but the disease to which these authors have affixed it, comes more properly under another appellation (Leucophlegmasia); and the term Cachexy is more properly employed by Sauvages and Sagar for the name of a class. In this I have followed the last-named nosologists, though I find it difficult to give such a character of the class as will clearly apply to all the species I have comprehended under it. This difficulty would be still greater if, in the class I have established under the title of *Cachexies*, I were to comprehend all the diseases other nosologists have done; but I am willing to be thought deficient rather than very incorrect. Those difficulties, however, which still remain in Methodical Nosology, must not affect us much in a treatise of practice. If I can here properly distinguish and describe the several species that truly and most commonly exist, I shall be less concerned about the accuracy of my general classification; though, at the same time, this, I think, is always to be attempted; and I shall pursue it as well as I can.”—“ It is not possible,” he remarked in his lectures, “ to say any thing more on the class generally considered. It is artificial, comprehending diseases which have little connexion with one

another. I give, therefore, very little of an introduction to either the class or the orders; but if we keep in view what belongs to particular diseases, we need be less anxious about the establishment of classes and orders."

It is much to be regretted that, in constituting a class of diseases intended to comprehend the derangements of the natural functions, Dr Cullen had not followed out more strictly the usual division of these functions into nutrition, secretion, and reproduction; for the loss of any advantage that can accrue from his definitions of the class and of its orders, would have been amply compensated for by the better approximation which he might thus have effected of resembling and related diseases.

Dr Good considers by far the most faulty and incorrigible part of Dr Cullen's arrangement to consist in his fourth and last division or class Locales; and Dr Young affirms of this class, "that it appears to be wholly undistinguishable by any sufficient criterion from general diseases."

In establishing a particular class of Local diseases, Dr Cullen remarked, in a note on his nosology, that it may sometimes, perhaps, be doubtful whether certain diseases ought to be referred to the universal or to the local; but that, in most instances, their proper reference in this respect will be sufficiently obvious; "and we maintain," adds he, "that by the institution of this class of local diseases, a very considerable number may be arranged more correctly and more easily than in the systems of other nosologists."

Dr Cullen has been much blamed for having introduced into the class Locales many diseases which, as

being affections of the whole system, and requiring a corresponding general treatment, ought not to have been so designated. The diseases referred to, seem to be particularly those in which there is affection of the nervous system; and Dr Hosack takes credit to himself for having introduced into his class Neuroses the paralyses of the senses, as well as several other diseases of the nervous system, which were, he says, “most strangely placed by Dr Cullen in his class Locales; as Nostalgia, which, being a form of Melancholia, is now associated with the Vesaniæ.” Dr Good also adverts to the difficulty in determining “what could be Dr Cullen’s motive for placing Nostalgia in any part of the class Locales.”

The considerations which guided Dr Cullen in the arrangements to which these objections seem to refer, are to be found explained in the notes appended to the general titles of the order Vesaniæ, the class Locales, and its orders Dysæsthesiæ and Dysorexiæ. In respect of mental affections, he was very desirous to draw a distinct boundary between those delusions and desires which arise solely and simply from morbid conditions of the sensory and other *organs*, and those in which there is a derangement of the *judgment*. It may here be remarked, that the special object of some recent ingenious memoirs by the late eminent mental pathologist M. Esquirol, was the marking of the same distinction, by applying the term hallucinations to imaginary perceptions originating in the mind; and that of illusions to false perceptions, dependent on the state of the bodily organs. (Arch. Gener. 2<sup>e</sup> serie vol. i.) Having in the first edition of his nosology defined the



Vesaniæ merely as “ Impaired Functions of the Mind, without Pyrexia or Coma,” Dr Cullen subsequently amended it by the introduction of the qualifying term *judging* mind, so as to exclude the false perceptions or hallucinations (the illusions of Esquirol), and the erroneous appetites or Morositates which Sauvages, Sagar, and Linnæus had, under various titles, combined with the diseases in which there is impairment of judgment.

“ It is true, indeed,” he remarks, “ that both false perceptions and erroneous appetites are sometimes conjoined with derangements of the judgment ; but in such cases, there are at the same time present other indications of the deranged judgment, which shew that these erroneous perceptions and appetites are only symptoms of a more general affection. For these reasons, I formerly ranked the Hallucinationes of Sauvages and the Imaginarii of Linnæus among the local diseases ; and I now do the same with the Morositates of Sauvages and the Pathetici of Linnæus, which I had formerly altogether omitted.”

He seems, however, to have become satisfied that he had drawn the line too strictly between the false perceptions and erroneous appetites that can be regarded as general affections, and those that are of local origin. “ I had excluded the Hallucinationes and Morositates from the order of Vesaniæ,” he remarked in his lectures, “ in so far as they depend upon a fault or disease of the external organs ; and I now perceive that I should have retained them more distinctly in so far as they depend on the brain itself. I truly do this, but not explicitly enough.” (Works, vol. i. p. 518.)

The mystery of the place assigned by Dr Cullen to

Nostalgia, is explained in a note on the *Dysorexiæ* or False Appetites, where, in justifying the propriety of placing these under the *Locales*, on the ground of almost all of them being manifestly affections of a single part, rather than of the whole body, he adds ; “ Nostalgia alone, *if indeed it be really a disease*, cannot by any means be considered as local ; but I could not well separate this uncertain disease from the other erroneous appetites.”

The same remarks as have been made relative to the *Hallucinationes* and *Morositates*, are applicable in regard to the place among the *Locales* assigned by Dr Cullen to the affections characterised by impairments of sense and motion (the first and third orders of the class, viz. the *Dysæsthesiæ* and *Dyscinesiæ*). By the definition he had given of the class *Neuroses*, he had excluded from it such nervous diseases as are accompanied by any local affection ; but from the commentary which he was accustomed to give on this definition in his lectures, as well as from his history of the particular diseases which he included under it, it is obvious that the local affections here alluded to are affections of parts external to the nervous system.

“ The Nerves,” he remarks, “ are the foundation of the animal economy ; they are the principal organs in every exercise of motion that occurs. But we perceive that the senses and motions of the different parts depend partly on some function of the origin of the nerves, and partly upon certain organs that are applied to their extremities. Now, the interruption of sense and motion may arise from a fault in the origin of the nerves, or from a fault of the organs appended to them. Those affecting the appended organs are local affections, and are to be excluded here ; and under this title

of Neuroses we are to give only the affections of sense and motion that are properly affections of the general system of the nerves. But we consider the affection of the nerves in their origin, or what we call the centre of the Nervous System."

The distinction which he marks between Palsy, considered as a local and as a systematic affection, clearly shews by what principles he was guided in referring affections of the powers of Sense and Motion to the class of General and to that of Local diseases respectively.

"The loss of the power of voluntary motion," in which this disease appeared to him to consist, "may," he remarks, "be owing either to a morbid affection of the muscles or organs of motion, by which they are rendered unfit for motion, or to an interruption of the influx of the nervous power into them, which is always necessary to the motions of those parts that are under the power of the will. The disease, from the first of these causes, as consisting in an organic and local affection, we refer entirely to the class of local diseases. I am here to consider that disease only which depends upon the interrupted influx of the nervous power; and it is to this disease alone I would give the appellation of *Palsy*. A disease depending on an interrupted influx of the nervous power may, indeed, often appear as merely a local affection; but as it depends on an affection of the most general powers of the system, it cannot be properly separated from the systematic affections."

In admitting the justness of Dr Cullen's distinction between those affections of the animal functions of sensation, voluntary motion, and thought, which depend on morbid conditions of the central portions of the nervous system, and those which depend on morbid conditions of the extreme ramifications of that

system, or rather of the organs exterior to these, it must be allowed to be very undesirable that these two sets of affections should be so widely separated from one another as they are in Dr Cullen's nosological arrangement. A similar observation applies to other two orders of this class, viz. the Apocenososes or fluxes without pyrexia or increased influx of fluids, and the Epischeses or suppressions of the excretions. By having found places for these, as might easily have been done, under some of the other classes, and by having omitted the three other orders of his Locales, the Tumores, Ectopiæ, and Dialeses, which have more relation to surgery than to the practice of physic, Dr Cullen would undoubtedly have obviated much censorious criticism.

The circumstance of Dr Cullen's having subjoined to his Nosology a list of diseases, about forty in number, which he had omitted in the body of the work, but which, he thought, ought perhaps to have been included, has furnished to some of his critics a handle for very severe animadversions. Dr Young declares that "nothing can be a stronger proof of Dr Cullen's genera, orders, and classes, being lamentably deficient in the essential qualities of a logical systematic method, than the numerous list of diseases which, from the defective constitution of his classes, he has been obliged to insert in an appendix, having no place in the system to which they could with propriety be referred;—an imperfection," he adds, "which not only implies a want of a clear view of the relations of the diseases immediately concerned, but a radical error in the fundamental divisions of the whole subject, which cannot have been established on natural grounds, while any

part of it is thrown out of the general order, as if it had no connexion with the remainder." Dr Good agrees with Dr Young in considering the necessity of a list of omissions as "fatal to the reputation of a nosological system." "The utter want of fit places for well known diseases in a nosological system," says he, in reference to a statement of Dr Cullen's presently to be quoted, "and this, too, in the opinion of the author of the system, is a defect from which no time or labour can ever relieve it." P. 18.

Dr Cullen, in his *Prolegomena*, after enumerating the means by which he had reduced the number of particular diseases in his *Nosology*, especially by throwing out such of those recognised by his predecessors as were in reality merely symptoms, goes on to remark ;

"I shall perhaps be readily pardoned for omitting the diseases I have mentioned. But there are other diseases worthy of being mentioned, the omission of which will not be so readily pardoned by the student. Omissions of this kind I confess and regret, but various reasons have caused me to omit several diseases. In the first place, it may be that some have entirely escaped our observation ; secondly, there are some sufficiently known, for which a fit place has nowhere been found in our system ; and, lastly, there are others whose history is so imperfectly known by physicians, that no fit place or character can be assigned to them. In order, however, that genera of this kind might not be omitted entirely, I have subjoined a catalogue of the *Omissi*, as far as I know them, in order that my more perspicacious followers, considering them with greater diligence, may assign to them characters and a place." "I know of some genera," says he in his *Introductory Lecture*, "which in my *Synopsis* are omitted, as I either had no clear or accurate knowledge of them, or could not work them into any part of my

system. When Ray attempted his system of botany, he found that a number of plants would not incorporate ; and hence he found it necessary to add at the end a list of anomalous plants, or, as Linnæus has it, *incertæ sedis*. I once thought of subjoining a few of that kind ; but, as I am not sufficiently acquainted with many of them, I have omitted them." (Works, vol. i. 461.) " The nosologists who have gone before me, have done well in attempting to bring every kind of disease into their system. But I find it extremely difficult to do the same thing : and, both from want of confidence in my own knowledge, and from want of leisure, I have not attempted to comprehend the whole of diseases, because I think it better to leave the general plan imperfect, than to introduce into it any thing about which I am uncertain. While I only attempt subjects of which I have a knowledge, I stand a better chance of escaping many errors, than those who have grasped at every thing, trusting not to their own knowledge alone, but more commonly to that of others." P. 458-9.

Botanists, it is believed, will not readily allow that the long list of plants which M. Jussieu has appended to his *Genera Plantarum*, under the designation of *Plantæ incertæ sedis*, exposes his methodical classification to censures similar to those which Drs Young and Good have so unsparingly pronounced on that of Dr Cullen. It is curious that they should have neglected to observe the oversight which Dr Cullen committed in not giving definitions of such of the diseases included in his list of *Omissi*, as he himself had had an opportunity of observing ; for this, it is obvious, he might have done, though unable to assign to them appropriate places in his methodical arrangement.\*

Dr Cullen added greatly to the value of his Synop-

\* Appendix C.

sis of Nosology, by introducing under the genera and species accurate and, in some instances, very copious lists of the synonymes employed both by other nosologists and by the most celebrated practical authors, with references to the corresponding definitions in the systems of Sauvages, Linnæus, Vogel, and Sagar, and to the best descriptions of each disease, as contained in the ablest systematic works or particular treatises that had appeared previously to the time of its publication. "I have done this," he says, "with the greater diligence, that students might thence learn from what writings chiefly a knowledge of diseases may be best attained. I have hitherto," he continues, "omitted the synonymes of the ancient physicians, not because I thought their writings should be altogether neglected, but because I could not point out their synonymes with sufficient certainty." This omission in his Nosology, of references to the ancient writers on medicine, has given occasion to several harsh and most unjust criticisms. Thus it has been said, "that, both by precept and example, Dr Cullen discouraged the study of the ancient medical authors; in his whole System of Nosology, where there occur upwards of six hundred citations, only three authors who wrote before the decay of literature are mentioned, and each of these but once;" and M. Roussel, in his Eloge of M. Bordeu, says, that "as to Cullen, it is easy to see that the ancients were for him as if they had never existed."

When treating of intermittent fever in his lectures on the Practice of Physic, Dr Cullen was accustomed to make the following remark with regard to his omission of the synonymes of the ancients.

“ If I could have given the synonymes of the ancients, it would have been of use to you in your after studies ; but the ancients are not precise, nor are they always uniform and constant, so such references would have been a little perplexing. And if I had taken it upon me to determine them here, I would have done it with doubt, and besides it would have cost me more labour than I could well bestow upon it. So I have abstained from giving any reference to the ancients, except through some modern. But if any one would more particularly consult the ancients upon this subject, he may do it in the works of Sennertus, who was a man of great erudition, and of a systematic turn ; and every student ought, at one period or other, to study the writings of Galen.”

Dr Cullen's opinions with respect to the amount of advantage to be derived, particularly by students, from the perusal and study of the ancient writers on medicine, were very explicitly stated by him in the following passage of a lecture introductory to his course on the Practice of Physic.

“ I acknowledge that the ancients may be read with advantage, but, at the same time, I maintain they are not to be read by a beginner in the study of physic, and at no time with so much advantage as has been commonly supposed. To take one example for all. The works imputed to Hippocrates are many of them the works of different men in different ages, and we have much difficulty in distinguishing the genuine from the spurious. We have reason to believe that even the genuine are mutilated, corrupted, and interpolated in many places. In the most entire the meaning is frequently obscure, and we are often at a loss to determine what are general and universal propositions, and what are particular and local facts. The reading of such writings must certainly be ill suited to a beginner in study. No one



is in a condition to attempt it till he has acquired a modern system of physic, and a large acquaintance with diseases; and it appears to me extremely ridiculous for any student to be very early engaged in the study of the ancients. For what I have said chiefly with respect to Hippocrates, if time allowed me, I could equally well apply to every one of the ancient writers now remaining. They cannot properly give any rudimentary or fundamental knowledge, nor can they ever give any complete knowledge of physic.

“ Sir Richard Blackmore, who has been so much the butt of the critics in poetry, was a physician. It is said that when he was beginning the study, he consulted Dr Sydenham about what books he should read to acquire knowledge; and that Dr Sydenham advised him to read Don Quixote. The meaning of this advice is not well understood or agreed on; but I judge it to imply that, in Sydenham’s opinion, the knowledge of physic was not then to be acquired by reading; and I do believe that Dr Sydenham thought there was not much to be learned by a young beginner from the perusal of the incomplete and obscure works of the ancients, or, indeed, of most of the writers before his time; and it is certain that he made little use of any of them himself.”

The general impressions which Dr Cullen’s lectures and writings relative to nosology produced on the minds of his more intelligent pupils, may be judged of from the following estimates of their value given by two of these, the late Dr Currie of Liverpool—a physician not more distinguished for his knowledge of practical medicine, than for the soundness of his philosophical opinions and the elegance of his literary compositions,—and the late Sir Gilbert Blane, whose constant aim during the long and active services in which he had been engaged, was justly said by himself to have been to extend the utility and uphold the dignity of the

medical profession, by founding it on the deep and solid basis of genuine science and sound philosophy.

“With more comprehensive views than Sauvages,” observes Dr Currie,\* “a more lucid order, and a happier simplicity, Dr Cullen divided the whole body of diseases into four classes and twenty orders. In his definitions, he excels in accuracy all who have gone before him; and it is, indeed, his distinguished and peculiar praise, that, not only in his Nosology, but in his First Lines, his descriptions of diseases receive no colouring from his theories, but are everywhere faithful to nature. Original and inventive, Dr Cullen, in his reasonings and explanations, dwelt much on the causes of diseases. Aware, however, of the imperfection of the art, he did not attempt to arrange them according to their proximate causes,—as it was his wish to have done,—but according to an humbler method, founded partly on their symptoms, partly on their causes, and partly on their seats. The Nosology of Dr Cullen has not had the attention or the praise it merits. The elder class of physicians were not likely to receive a new system from their contemporary, and the attention of the rising generation has been too soon withdrawn from this as well as the other works of this accurate observer by the bold and specious, but presumptuous and sometimes dangerous, speculations of his pupil Dr Brown. The Systema Nosologicum of Dr Cullen is, in the judgment of the writer of this article, his most masterly production, and, indeed, the finest synopsis of the history of disease that has hitherto issued from the press.”

“Dr Cullen’s Nosology,” says Sir Gilbert Blane, in a let-

\* Dr Currie entered on the study of medicine at Edinburgh in the winter of 1777-78. “His indefatigable industry as a medical student,” says his biographer, “attracted the notice of the different Professors; and amongst these he was distinguished by the flattering kindness of the illustrious Dr Cullen. To this great man he some years afterwards paid the following [*vide supra*] testimony of respect and admiration, in his Review of Darwin’s Zoonomia.” (English Review, Lond. 1796, p. 537.) See Life, p. 43, 45.

ter with which he favoured me on his last visit to Scotland in 1828, "is a proof of his clear and comprehensive mind ; for with all the faults and imperfections which seem inseparable, by its peculiar nature, from the subject, it is superior to any of the systems which preceded, and equal to any which have followed it."

That opinions relative to the value of Dr Cullen's nosological labours, of a very opposite nature to those which have just been quoted, have, as we have seen, been expressed by various authors of considerable reputation, must be attributed, I conceive, partly to the very mistaken notions which they have entertained as to the purposes which Nosology is intended to serve, or the objects which fall under the limits of its investigations, and more particularly as to the relative importance of its several departments ; and partly also to a gross inattention to the exposition which Dr Cullen has given of his own views in this department of medical science, and of the principles by which he had been guided in endeavouring to carry these views into effect. For a large share of the objections that have, since his time, been advanced against nosology in general, and against his own system in particular, had previously been candidly pointed out and most ably discussed, if not refuted, by himself, in the Preface or in the Notes to his Synopsis.

Respecting those objections, in particular, that have been urged to that department of nosology which relates to the distinction and definition of diseases, it may be observed, that they seem to be founded either, 1st, on the difficulties which attend the advancement and application of medical science in general, rather than

of this branch of it in particular ; 2*d*, on its imperfect state, rather than on its incapability of improvement ; or, 3*d*, on the abuse which has been made of it, rather than on its being destitute of use. They seem to have been propounded, also, in forgetfulness of the truth so well expressed by Dr Cullen in his remark, that “perfect division and definition is the summit of human knowledge in every department of science, and requires not only the clearest but the most comprehensive views, such as, with respect to diseases, we can arrive at only by often repeated exercises and much study.”

I cannot conclude the consideration of Dr Cullen’s labours in Nosology in more appropriate terms than those employed by that distinguished pathologist, M. Bayle, in speaking of the difficulties necessarily attendant on nosological definitions and classifications. “In waiting for a perfect guide,” says he, “which perhaps we shall never have, let us follow the plan which presents fewest imperfections ; but let us not forget that the determination of the species is what is most essential, and that the arrangement is what is least important in a Nosology ; that each arrangement has its defects, presents its deficiencies, and exhibits some forced approximations. Let us appreciate the plan of arrangement at its just value, let us consider it as a repertory more or less exact, and let us prefer the one which shall bring together the greatest number of analogous diseases.”

In the year 1770, an alarm arose in Scotland that an infectious distemper which was prevailing among the Horned Cattle in Holland, and which, between

April 1769 and March 1770, had destroyed in that country nearly 160,000 cattle—more than two-thirds of those which it attacked\*—had made its appearance at Portsoy, a sea-port town in the Moray Firth. Very active measures were adopted, both by the local authorities and by government, for checking the progress of this malady which, eighteen years before, had proved so destructive to the horned cattle in England, as to call for Parliamentary investigation, and had given rise to several interesting publications. On this occasion, Dr Cullen was applied to by the law-officers of the Crown in Scotland, to suggest what he thought might be useful. In compliance with this requisition, he drew up a short “Memorial concerning the Contagious Disease affecting the Horned Cattle,”

\* This was the epidemic which afforded to the celebrated Camper occasion to deliver in 1769, in the Anatomical Theatre at Groningen, those lectures on the Pestilential Disease of Cattle which he subsequently published;\* as well as to make his experiments on the Inoculation of the cattle distemper, by the results of which he was led to believe that the employment of this practice during the prevalence of an epidemic, is calculated to diminish the mortality occasioned by it.† The “Impartial Examination of the advantages which the inoculation of the Epizootic disease has produced in Holland and Germany, and of those which may be expected from it in France,” published by M. Vicq d’Azyr in the History of the Royal Society of Medicine for 1777-78, seems, however, to have satisfied those interested in the investigation of the diseases of horned cattle, that the practice of inoculation can never hold a principal place among the means for preventing the ravages of the malady in question.

\* See Œuvres de Pierre Camper, &c. Paris, 1803. Tome iii. p. 7.

† De l’Inoculation de l’Epizootie, de ses avantages et des précautions qu’elle demande. Ibid. p. 178.

which, along with an order of the Privy Council on the subject, and other documents, was transmitted to the several sheriffs of the district. Assuming with Lancisi, to whose treatise on this disease, as it appeared in the Pope's territories in 1711, he refers, that the disease could be very readily propagated, not only by the direct communication of infected with sound cattle, but indirectly by substances which had been in contact with diseased animals, Dr Cullen's object in this memorial was to point out the less obvious modes in which the infection might be communicated, as by fomites of various sorts, and to suggest the means of obviating these. Though the disease was supposed to shew itself at several places in the neighbourhood of Portsoy, during the months of March, April, and May, it never spread extensively,\* a fact which seems to afford a presumption at least, that the means employed there were beneficial in preventing its diffusion.

Very soon after this, another occasion for public alarm presented itself, which again called Dr Cullen's attention to the consideration of the measures by which the spreading of a malignant contagious disease may be most effectually resisted or controlled, an apprehension, namely, of the probability of the Plague being introduced into this country.

It is well known, that a war having broken out in 1769 between the Russians and the Turks, the troops of the latter nation carried the plague along with them into Wallachia and Moldavia. This pestilence, after

\* Scots Magazine, 1770, vol. xxxii. p. 172.

spreading, in the course of the following summer, into Poland, passed by Kiou to Moscow, which it reached about the end of the year 1770. In that city it continued to prevail about a twelvemonth, during which period it destroyed from 70,000 to 80,000 of the inhabitants. On the 5th October 1770, orders of Council were issued in England for the performing of quarantine, in consequence, as they bear, of information having been received that the plague had broken out, and was then raging in Wallachia, Podolia, and other parts of Poland; and the apprehension thereby excited, that the infection might be brought into this kingdom from Dantzic, or some other place in Royal and Ducal Prussia or Pomerania.\* The appearance of this fearful disease in the same latitude with their own city, very naturally alarmed the inhabitants and civic authorities of Edinburgh, who applied to the members of the Medical Faculty in the University for advice as to the measures which it might be proper to adopt, with a view to prevent the introduction of the plague into Edinburgh, or to diminish as much as possible its destroying influence in the event of its actually manifesting itself there. The answer of the Medical Faculty seems to have been prepared by Dr Cullen, among whose papers I find several revised copies, in his own handwriting, of a "Memorial concerning the administration of the City of Edinburgh, in case of its being in more immediate danger of the Plague, or of its being actually infected with it;" as well as the final copy signed by himself and his colleagues. The memorial exhibits a clear and concise detail of those

\* Scots Magazine, xxxii. 532.

measures which are proper to be pursued in a city or district in the dreaded or actual attack of any epidemically contagious disease ; but, fortunately, there did not occur at that time any occasion for putting into execution the measures which it suggested.

In August 1773, on the resignation of Dr Colin Drummond, who had gone to settle at Bristol, Dr Cullen was elected, by a vote of the College, President of the Royal College of Physicians. In this situation, which he occupied till November 1775, a variety of duties devolved upon him. The College, in 1774, published a new edition of their Pharmacopœia, on the composition and arrangement of which it appears, from their Minutes and from manuscript notes in my possession, that Dr Cullen had bestowed, for some years previously, a great deal of pains. Works of this kind being intended for the guidance of the dispensing druggist or apothecary, as to the articles with which his shop should be supplied, it seems to have been long customary to divide them into two parts, the first intended to exhibit a list of those substances which, either from their being used in their natural condition, or from their being supplied ready made by the wholesale dealer, do not require any preparation at the hands of the apothecary ; and the second part, containing an enumeration of those substances which it is necessary for the apothecary himself to prepare, with instructions as to the mode of preparing them. The existing state of the chemical arts must at all times determine, in respect of a large number of the compound articles employed in medi-



cine, whether they ought to be referred to the one or to the other of these classes of substances ; since substances, which at one time were seldom if ever used except as medicines, and which, therefore, it devolved upon the apothecary himself to prepare, have at subsequent times come to be extensively employed in the practical arts, and their preparation has become, perhaps, the proper or exclusive employment of particular manufacturers.

In endeavouring to adapt their Pharmacopeia, the previous edition of which had been published in 1756, to the existing state of medical practice and of the arts of Chemistry and Pharmacy, the College of Physicians made several alterations on both parts of the work. In the first part, they shortened the catalogue of medicinal substances, by omitting such as had fallen into disuse, and such as, from their being articles of domestic use, it is unnecessary for the apothecary to keep ; but to this list they transferred, from the second part of the work, several compound substances which the apothecary can procure better from the manufacturer than he can, himself, prepare them. And, with a view to introduce more precision into the vegetable department of Pharmacy, they added to the names of the different plants enumerated in the catalogue, the specific appellations employed by Linnæus. In the second part of the work, too, they found occasion to omit a number of medicines admitted into former editions, but which either had ceased to be employed by medical men, or were to be regarded as more properly the subjects of *pro re nata* prescriptions than of pharmaceutical formulæ. Some new medicines were introduced

which, either from the personal experience of the members of the College, or from the credible authority of others in their favour, seemed to promise to be useful in the practice of medicine, and an attempt was made to render the chemical nomenclature more accurate and simple, by divesting it of many of the terms which the early chemists had employed, either from ostentation or from a desire of mysterious concealment. No lengthened examination can now be required of a work which, from its very nature, must, in a few years after its publication, have become in a great measure obsolete. It is sufficient to have shewn that the edition of the Edinburgh Pharmacopœia, published under Dr Cullen's superintendence, fully kept pace with the progress of medicine and its collateral sciences, and was calculated to do credit to the body from which it emanated.\*

Besides receiving the suggestions of his resident fellow-members on the subject of the Pharmacopœia,

\* In proof of the estimation in which this edition of the Edinburgh Pharmacopœia was held, it may be mentioned that, besides a first impression of 2000 copies, a second edition of 1000 copies was agreed to in September 1775, which, by May 1778, was so far exhausted as to occasion an application from the publisher for permission to throw off other 2000 copies. It was reprinted at Rotterdam in the year subsequent to its publication at Edinburgh, and two editions of it were published in Germany under the superintendence of Professor Baldinger of Göttingen, the first in 1776, and the second at Bremen in 1782. The learned editor, in a lengthened preface prefixed to the first of these editions, speaks of it as "*emendata omnino, et sapienti concilio contracta, ac passim novis medicamentis aucta, ut jure meritoque perpolita nuncupari possit, suisque auctoribus, viris eruditissimis, sagacissimis, rerum usu peritis, perquam digna.*"

Dr Cullen corresponded with Sir John Pringle, who was a fellow both of the Edinburgh and of the London College of Physicians, and who at the time was President of the Royal Society of London. Sir John's letters contain a number of minute criticisms both on the proper matter of the Pharmacopœia, and also on the dedication to the King, the adjustment of which seems to have been a matter of considerable difficulty. In receiving at last copies of the published work, Sir John Pringle expresses himself in regard to it in the following terms (19th August 1774):

“ I hope the work will tend to the advancement of the reputation of our society; though perhaps not so much immediately as hereafter, when the rest of the medical world comes up with it, by acquiring such a degree of learning and good sense as may enable them to see and acknowledge the barbarity and absurdity which hitherto have reigned in almost all the works of this kind. I should judge that in point of simplicity and elegance of composition, where composition is required, the new Edinburgh Pharmacopœia has got as far before the last London Pharmacopœia,” that published in 1746, “ as that work excelled all others preceding it. Doubtless, however, we shall have fault found with several things, by some for having so far cast off the old farrago, and by others for not having availed ourselves of all the new lights. To the latter set of critics we shall be obliged, as their remarks may be turned to profit by those of us that shall be alive at the publication of the next edition.”

Another matter connected with the College of Physicians, into which Dr Cullen entered very warmly, was the arrangements relating to the building of their present Hall. So early as 1756, the year of Dr Cullen's entrance into the College, the building of a new

hall for its meetings had been under contemplation. The College at that time had their meeting-room situated in the middle of a garden near the Cowgate Port; but the house having become ruinous, they did not choose to build in that situation. Their property having been bought in 1770, by the gentlemen of the Episcopal communion in Edinburgh, for the erection of a chapel—the Cowgate Chapel—the College, in the subsequent year, appointed a committee to consider of ways and means for purchasing a proper area and building a new hall. No very active steps, however, seem to have been taken till the presidency of Dr Cullen, who, early in 1775, reported matters as in an advancing state. Arrangements were promptly made with the Town Council for the area in the centre of one of the divisions of George Street, on which the hall now stands; a subscription was set on foot among the members; and before Dr Cullen's presidency terminated, he had submitted to the College a plan of the proposed new hall, designed by Mr Craig, architect—the nephew of the author of the Seasons, and the planner of the New Town of Edinburgh—which had been approved of by architects of reputation both at London and at home. By a letter from Sir John Pringle to Dr Cullen, it appears that the plan had been submitted to Mr Stuart, the author of the Antiquities of Athens. Out of compliment, I presume, to Dr Cullen, the College recommended to the Building Committee that the foundation should be laid at farthest before the end of November, that is, before the expiry of his presidentship; and, accordingly, the ceremony took place on the 27th of that month. On the 7th

May 1776, on Dr Cullen's reporting the arrangements with Mr Craig as ready for ratification, the unanimous thanks of the College were, upon a motion from the then President, returned to the Building Committee, and particularly to Dr Cullen for his very great and unwearied labours in forwarding and finishing the above business. These labours appear to have been continued during the progress of the building, till the College met in its new hall for the first time in August 1781.

As connected with the College of Physicians, there is another matter in which Dr Cullen took a part that may here be noticed. In all periods of the existence of medical incorporations, the harmony of these institutions has been frequently disturbed by attempts to draw a broad line of distinction between different branches of practice, as being more or less dignified or reputable in their nature ; and to restrict the privilege of admission into particular corporations, to persons who devote themselves exclusively to the exercise of particular departments of the healing art. The College of Physicians of Edinburgh has, in its time, had its full share in the contentions arising out of such attempts. Its friends may congratulate themselves on these restrictions having been abandoned voluntarily, and at a more early period than by some kindred institutions ; and it is satisfactory to find that Dr Cullen entertained just and liberal views upon this subject.

In 1754, the College had passed an act prohibiting their Fellows or Licentiates from taking upon themselves to use the employment of an apothecary,

or to have and keep an apothecary's shop. In 1765, in order, as they conceived, "to support that character and esteem which they had all along maintained, and to keep up that distinction which ought to be made between the members of the College and the practitioners of those branches of the healing art which have been always esteemed the least reputable," they resolved that for the future they would admit no person to be one of their *Fellows* whose common business it was either to practise Surgery in general, or Midwifery, Lithotomy, Inoculation, or any other branch of it in particular. This resolution was followed up by a proposal to make a law extending the same restriction to *Licentiates* of the College, which seems to have remained under the consideration of the College from May 1765 to February 1769, without receiving any vigorous opposition. But, at the latter period, an act declaring those who practised Surgery or any of its branches disqualified for being admitted *Licentiates* of the College, having been agreed to by a majority, a dissent from this determination was entered by Dr Thomas Young, who, from the year 1756, had held the office of Professor of Midwifery. In this dissent he was supported by his colleagues in the University, Drs Cullen, Monro, Ramsay, John Gregory and Black, and by Dr James Hay. In the further progress of the discussion, which lasted till May 1772, when the College reverted to their original resolution of prohibiting the practice of Surgery and its several departments by *Fellows* of their body only, Dr Cullen took, in a great measure, the lead at the meetings of the College. Two very elaborate papers on the sub-

ject were lodged by the dissenters from the College's resolution, to which Dr Cullen's name is the first subscribed, and which probably were, in part at least, if not chiefly, composed by him. A few passages from these, which appear to me, in conception and expression, worthy of his acute and vigorous mind, will be found in the Appendix (D). An able paper on the other side of the argument is signed by Sir Stuart Thriepland, who was President at the time.

Dr Cullen had the pleasure, a short time before his death, to see the obnoxious resolution of the College repealed, in so far as it prohibited the Fellows from the practice of midwifery; and he availed himself of this repeal, at the second last meeting of the College which he attended, to propose for admission to the Fellowship the late Dr Charles Stuart, whose inaugural dissertation *De Systematis Nervosi Officiis*, published in 1781, shews how much pains he had taken in familiarising himself with the doctrines of his preceptor. This proposal of Dr Stuart, as a Fellow, Dr Cullen said was a measure he had always intended, as soon as the laws of the College would permit.

In 1774, Dr Cullen drew up a Letter on the Recovery of apparently Drowned persons, addressed to Lord Cathcart. A society had been formed at Amsterdam, in the year 1767, for the recovery of drowned persons.\* This laudable example having been followed in Milan, Venice, Hamburg, and Paris, a society for the same purpose, which still exists under the name of the Hu-

\* See "*Histoire et Mémoires de la Société formée à Amsterdam, en faveur des Noyés.* 1767."

mane Society, was instituted at London in the summer of 1774. The attention of Lord Cathcart, who at that time held the office of President of the Board of Police in Scotland, having been directed to the subject, he transmitted a "paper relative to persons drowned and recoverable, though seemingly dead, with directions proposed to be promulgated in the several counties and burghs in Scotland," to Dr Cullen, for his opinion as to its suitableness. Dr Cullen considered the directions proposed to be promulgated as very judicious, and perfectly adapted to the purpose intended; but thinking that advantage might result to the public from entering a little further into the explanation of principles and the details of practice, he embodied his views on the subject in a letter to Lord Cathcart, and the two documents having been laid before the Board of Police, were (11th August 1774) ordered by them to be printed and extensively distributed throughout Scotland.

The notion was long entertained that, in drowning, death is occasioned by the entrance of water into the stomach and lungs; and it followed as a practical deduction from this belief, that among any measures employed for the recovery of drowned persons, those calculated to promote the evacuation of this water must hold a principal place. Dr Cullen was at pains to enforce the erroneousness of the explanation, and the injurious consequences that must result from the preposterous plan of treatment founded upon it.

The measures proper to be pursued in the management of drowned persons he reduced to a few heads. His first indication was to endeavour to restore the



heat of the body ; and he described the different proceedings to be followed for this purpose. His second indication was to restore the action of the moving fibres, which he conceived might be most advantageously effected by throwing tobacco-smoke into the intestinal canal, a practice in general use at that time, and on the efficacy of which great reliance was placed, but which seems now to be very properly abandoned as useless, if not detrimental, in consequence of its positively debilitating effects. His third indication was to restore the action of the lungs and heart, for which purpose he recommended inflation of the lungs ; and he has mentioned the results of some experiments made by his colleague Dr Monro, for the purpose of ascertaining the best manner of accomplishing this inflation. The light which, in the progress of chemical science, has since been thrown on the function of Respiration, has corrected the notion obviously entertained by Drs Cullen and Monro, that it is immaterial whether inflation be performed with air that has previously passed through the lungs of another individual or with unrespired air. Late observations and experiments, also, seem to suggest the necessity of great caution even in the performance of the mechanical part of insufflation, by shewing the injurious effects which are produced when this operation is performed with any degree of force. It may be doubted, indeed, whether alternate inspiration and expiration may not be accomplished as effectually and more safely by gentle attempts alternately to elevate and to depress the chest by the hands or by a bandage, than by any other method that can be employed.

In addition to the fulfilment of these three primary indications, Dr Cullen recommended as measures likely to prove beneficial, *1st*, The opening the jugular veins to relieve the congestion which, he says, almost constantly occurs in the veins of the head, and is probably a frequent cause of the death of drowned persons; and, *2d*, The application of certain stimulants to the more sensible parts of the body.

Notwithstanding the more extended opportunities that have been afforded by the formation of societies so properly established for the recovery of the drowned, the number of interesting facts that have been brought to light by the experimental investigations which have been instituted on the subject of drowning, and the more elaborate consideration to which, in the progress of physiology, the general doctrine of Asphyxia has been subjected, it seems to be doubtful whether any means more efficacious than those enumerated by Dr Cullen have yet been discovered.

From the time of his obtaining sole possession of the Chair of the Practice of Physic in 1773 to the period of his retirement in 1790, Dr Cullen's professional studies were mainly directed to subjects immediately relating to practical medicine, and to the means by which the knowledge of this branch might be most effectually imparted to his pupils. On the improvement of his practical lectures he bestowed unceasing pains; and in order to allow himself more time for the discharge of the duties of his Chair and of the other avocations arising out of his more extended personal prac-

tice and written consultations, he, in 1775, relinquished the teaching of Clinical Medicine.

The concern which he took in the instruction of his pupils was frequently gratefully acknowledged in addresses from them at the close of the session. In the introductory lecture to the course of 1782-83, Dr Cullen, who was then seventy-two years of age, mentions that, for two sessions past, he had given lectures at a separate hour to second and third years' students, "with what advantage," he remarks, "they must judge; but I believe it to be of service, and shall continue it." It is to these meetings obviously that the following address relates.

"SIR, .

"EDINBURGH, *March* 1782.

"Permit us to express the grateful sense which we have of your goodness in meeting with us at five of the evening, amidst your various and important avocations.

"You have generously dedicated to our improvement those valuable hours which, as usual, you would otherwise have employed in promoting the health of mankind, and in enlarging the boundaries of science and of the Medical Art.

"With a familiarity which endears you to us as a man, with an acuteness which characterises the Philosopher, you have explained to us those important doctrines which ought to be the guides of our future years.

"We perhaps shall soon visit the most distant quarters of the globe; but we trust that neither distance of time nor place shall ever erase from our memories the high obligations which we lie under to our disinterested and illustrious Professor.—Sir, we have the honour to be," &c.

That his advancing years did not diminish, in any degree, his zeal and assiduity as a teacher, may be in-

ferred from the statement which he himself gives in the following passage of the introductory lecture to the course of 1786-87.

“ That my course is not comprehensive enough, and does not treat of every subject that might be desired in a course of the Practice of Physic, is a matter that has always given me much concern ; but with all the pains I can take, I have never yet been able to do so much as I wished. From after the 1st of January I lecture six days of the week, and from after the 1st of March I give three afternoon lectures every week. This, I hope, will be allowed to be taking pains on my part; but, after all, I have not been able, nor shall be able, to render my course so complete as I could wish. The hinderance arises from the extent of the subject, which is larger than can possibly be comprehended in the time allotted to my course. At Bologna, the course of Practice takes up four years ; at Paris, it takes three ; at Leyden, it is an annual course, and takes up eight months, but in these eight months they do not go through the half of their subject.”

As a farther illustration of the zeal with which Dr Cullen was accustomed to discharge the duties of Professor of the Practice of Physic, it may be mentioned that he held regular oral examinations of his pupils. Among his papers I find a Latin letter of thanks addressed to him by a number of students, commencing “ Nobis quite non solum docentem, sed examinantem quoque audivimus,” which, from the names attached to it, seems to have been written in 1783. There is also another Latin letter preserved, written apparently the year after, thanking him for having held “ privatas conciones, ubi varias opiniones ad rem medicam pertinentes eo animi candore, eaque ingenii vi quibus emines, dijudicasti ; ubi errores autorum, non verborum asperitate, sed

*mira morum suavitate, qualis philosophum decet, redarguisti, et unumquemque te consulentem consiliis tuis exeristi et confirmasti."*

I am not aware that the practice of holding examinations of the students on the subjects treated of in the lectures delivered to them, had been followed by any medical Professor in this University previously to Dr Cullen ; nor, I believe, was his example imitated for many years. Oral examination is certainly one of the most difficult, but it is at the same time one of the most useful forms of academical instruction ; and it is one for which Dr Cullen was in many respects admirably fitted by that frank and kind manner which put his pupils so much at their ease in his presence ; by the conversational fluency which made his society at all times so agreeable ; and by the extensive knowledge he possessed in all the departments of medical science.

The chief insinuation that has been thrown out by those who have wished to depreciate the merits of Dr Cullen as a teacher of practical medicine, has been, that he himself indulged, and that he encouraged his pupils to indulge, in frivolous hypotheses, to the neglect of more solid practical inquiries,—an allegation sufficiently refuted by those correct histories of diseases, and explanations of the treatment they require, which his lectures exhibited, and the outlines of which he was at all times diligently employed in preparing for the public. The following extract from an introductory lecture, delivered at the commencement of the course for 1781-82, will shew how little foundation there is for such allegations ; and with how much earnestness he

enforced on his students the necessity of a minute and accurate attention to facts ; pointed out the fallacies of systems, and shewed that the proper use of hypothesis is to suggest inquiry, and to guide, but not to supersede, the investigation of facts.

“ On this subject (the general conduct of study), I shall offer you one lesson which, in my opinion, is the most important I can give you. It is this,—that, in the study of physic, your chief and most constant attention should be directed to obtaining the facts of physic, without which you cannot proceed at all,—and to obtaining on every particular subject the whole of the facts relating to it, without which you are in danger of proceeding erroneously.

“ To make this clear, I would observe to you, that almost the only mode of reasoning which medical discussions admit of, is what is called the method of Induction ; that is, from a number of facts relating to the same subject, as all agreeing in one particular, taken together, to form a general conclusion which we may employ as a principle in our after reasonings. If this is our method, it will readily appear that the truth of such conclusions and principles must depend upon the exactness of the facts, and especially upon the number of them being tolerably complete with respect to the subject of the conclusion.

“ Whoever knows anything of physic, and of the opinions which at different times have prevailed in the schools of physic, must know that many opinions which formerly prevailed are now universally exploded ; either because they were not founded on facts at all, or because they were founded on false and mistaken facts, or because they were general conclusions from a few facts only, while many others relative to the same subject were unknown or unheeded.

“ Such, gentlemen, is the importance of facts, and so much are these the foundation of our art, that I maintain that the state of physic, at any period of its existence, has

always been more or less perfect, precisely in proportion to the stock of facts which for the time had been acquired and attended to.

“ Just now, I shall only touch one general question with regard to my present subject of the study of facts. The question is, Whether we have yet got such a stock of facts as may allow us to attempt a system? I believe that some of the most knowing will assert that we have not; and I affirm very confidently, that we have not yet got so many facts as are necessary to form a tolerably complete and perfect system. None such, indeed, is to be hoped for; but I still maintain that a system, as well as we can make it, is to be attempted, for these reasons; *1st*, as the best means for collecting and remembering the facts already known; *2dly*, as the best means of rendering them in the mean time as useful as possible; and, *lastly*, as the best means of discovering where facts are wanting, and therefore of leading us to that diligence and attention in observation and experiment that are the only means of supplying our deficiencies. I admit, therefore, and I plead, therefore, for the utility of system, but still it is with a constant distrust of it as such, and that distrust, too, is always the greater in proportion as the system is more general.

“ How much soever, gentlemen, I may seem to employ a system, I assure you it is with a constant attention to the facts which form and establish it, and I hold that a system for particular parts of our subject, is more safe than any general one, because particular systems are, with me, always nearer to, and more closely connected with, the facts which establish them.

“ To conclude, gentlemen, ever since I have been engaged in teaching physic, it has been my study, from reading and experience, to collect the facts of physic; and, in teaching, to communicate them as fully as I possibly could. I may seem to maintain and deliver a system, but it is only

because I think it the best means of communicating the whole of the facts, and of applying them in the best manner I know of. For this session I am, for that purpose, to attempt a system; but, at the same time, I shall always give you the facts which may occasion any doubts of my system. I shall sometimes give you facts which may make me desert it altogether, and I shall often give you facts which, so far as I can judge, do not admit of any system at all. But I shall thus give you the whole of the facts, to be employed as your own judgment shall afterwards think best."

In the introductory lecture delivered by him in the subsequent year, in speaking of his own progress in medical philosophy, Dr Cullen says,

"In pursuing my studies, and constantly aiming at a complete system, I found that not only many errors had prevailed, but taking the systems offered at very best, I perceived that innumerable defects still subsisted in all of them. I felt all this in my teaching, and though I was always frank in acknowledging my own errors and defects rather than those of others, yet I must now fairly own that I never did either the one or the other as fully as I wished. I found criticism and controversy tedious, and I found that insisting on the imperfections of the art proved rather discouraging to my hearers. I must say, farther, that I have always found the ardour and boldness, or rather the temerity, of young genius was not to be restrained, as I thought there was little harm in speculation, because I expected that riper judgment would correct it; so I own that I have always indulged the pursuit of system both in myself and others.

"I have always felt great defects in the art, and many in my own endeavours to supply them; but I maintain that I have always been making some progress, and have never repeated a course without making both corrections and additions to that I had given before. I must own, however, that my progress has been often very slow. The system, '*quem animo metimur*,' to use Lord Bacon's phrase, is indeed of im-



mense extent, and is not to be reached in the course of one man's life ; and measuring matters in this way, I have indeed done very little ; but comparing with what had been done before, I flatter myself that I have done a good deal."

After having taught Clinical Medicine for sixteen years in the Royal Infirmary, and delivered six courses of lectures on the Practice of Physic in the University of Edinburgh, Dr Cullen commenced the publication of his *First Lines of the Practice of Physic*, a work which, as we have already seen (vol. i. p. 27), he had projected thirty years before. The first volume of this work appeared in 1776, the second in 1779, the third in 1783, and the fourth and last in 1784. Dr Cullen intended to have added a fifth volume on the Diseases of Women and Children, but the materials for it do not appear ever to have been arranged by him for publication.

Though various Systems and Compendiums of Practical Medicine had been published in different parts of Europe subsequently to the appearance of the Aphorisms of Dr Boerhaave in 1709, yet none of these seem to have acquired so great a reputation as that work obtained, or to have been employed so extensively either as text-books by teachers of medicine, or as guides for those engaged in the practice of physic. The views of the animal economy which had been inculcated by Stahl, Hoffmann, Haller, Whytt, Gaubius, and other physicians, differed, in many respects, so widely from those adopted in the writings of Boerhaave, and so great and rapid an increase of knowledge had taken place in all the branches of medical science, particularly in what relates to the influence

of the nervous system on the other parts of the animal economy in the states of health and disease, that a generally increasing conviction had been produced in the minds of medical men, of the want of a systematic work which should combine the improvements that had been made in the theory of medicine, with the practical results of experience in the treatment of diseases.

The length of time during which Dr Cullen had been zealously engaged in the study of both the theory and practice of medicine, his situation as Professor of the Practice of Physic in the University of Edinburgh, and the enthusiastic admiration entertained for his talents and medical acquirements by his friends and pupils, all seemed to point him out to the medical profession as the person in these kingdoms by whom an undertaking so difficult in its execution might be expected to be best accomplished. How fully the wishes of the public and the sanguine expectations of his friends were realized by the publication of his *First Lines*, may be presumed from the very favourable reception which this work met with on its first appearance, and from the extensive use that has since been made of it in the study and practice of medicine in all parts of the civilized world.

Soon after the appearance of the first volume of this work, a Latin translation of it was published at Leyden by Dr Boerenbroeck of Antwerp, who had studied medicine in Edinburgh under Dr Cullen. Several letters from this physician have been preserved which give a very pleasing account of the interest that had been excited by Dr Cullen's writings in the school

of Leyden. In one of these letters, dated 2d September 1777, he says,—“ After my arrival at Leyden, I waited on Dr Hahn (Professor of the Practice of Physic, &c.), who was so obliging as to offer to look over my translation of your First Lines, himself, and take care of the printing and correction of it. I waited also on Dr Gaubius, who had read with great satisfaction the English edition of your work ; though he says that if he had written it, he would have made another arrangement of diseases. He can well conceive, he says, how debility lays the foundation of the gout, according to your opinion of that disease ; but supposes an acrid is generally applied to the system, and is the *conditio sine qua non* of the gout. He is of opinion that the Latin edition will be read very much, and lead to many useful inquiries. On the whole, he thinks the First Lines very useful, and the general precepts you have laid down concerning bleeding, &c., excellent indeed. This is nearly the opinion of Van Doeveren also whom I went to see. He flatters himself that he has been in a great measure the cause why you have taken so much pains to refute the doctrine of morbid matter, which, however, according to him, has not been done in a manner altogether satisfactory. He desired me to stay a whole day with him to go over each argument separately, and I promised to do it at my return from Aix la Chapelle. Dr Hahn thinks that the work will be reprinted in Germany, and serve as a text-book in some of the Universities. Whatever be the fate of the book on the Continent, I shall be happy if the translation be to your satisfaction. Dr Hahn told Gaubius that he would write a preface to it ; but to me he mentioned nothing about it, except that he thought that I might add a great deal more to the Latin preface which I wrote at Edinburgh.”

In France, Dr Cullen's First Lines seem to have met with considerable favour. In the *Histoire de la Société Royale de Médecine*, an. 1777-78, in the no-

tice of works published by their members, the two first volumes are spoken of in the following terms : " It would be difficult to point out to students of medicine a work more clear, more methodical, and more solidly written. Throughout it bears marks of the skilful practitioner. All the facts conformably with which Dr Cullen gives the history, the etiology, and the treatment of diseases, are discussed and analyzed in it with the most uncommon sagacity. Conjectures are presented with a reserve and wisdom that characterise the exact and impartial observer. In the first volume will be read with the greatest pleasure, more particularly, the author's doctrines of fever, of crisis, and of critical days ; and, in the second, what relates to exanthematic fevers, and to pulmonary consumption."

In 1785, a French translation of the First Lines was published by the late distinguished M. Pinel. A copy of M. Pinel's translation seems to have been transmitted to Dr Cullen, by a mutual friend, for his remarks, and a scroll in his handwriting has been preserved, containing a considerable number of comments, which are interesting, as evincing a singularly critical acquaintance with the structure and peculiar idioms of the French language. In transmitting these criticisms to his correspondent, Dr Cullen observes,—  
" Though my leisure is very little at present, I have, at your desire, looked into M. Pinel's translation of my First Lines, and so far as I have gone, I am very well pleased with it. I hold it to be impossible to make a perfect translation. For one man to understand everywhere precisely and fully the ideas of another, written in a language foreign to him, and to express these clearly and correctly in his native tongue, is not to be expected ; and though M. Pinel has sometimes fallen short of my ideas, his translation is, upon the whole, very respectable, and I think myself highly honoured by the pains he has taken to translate my First Lines into French."

About the same time with, or very soon after, M. Pinel's translation, the learned M. Bosquillon published another French translation of the First Lines, to which he gave a great additional value by introducing into it the definitions of the genera, species, and varieties of diseases, as they had been given by Dr Cullen in his Nosology, and by adding numerous extracts from Dr Cullen's manuscript lectures, both on the Theory and the Practice of Physic, which M. Bosquillon, as he informs us in his preface, had read and studied for a period of twelve years. The character which M. Bosquillon has given of Dr Cullen's writings in his *Discours Préliminaire*, sufficiently proves how fully he understood the opinions, and with how much sagacity he appreciated the merits, of the author whom he undertook to introduce to the knowledge of his countrymen. "The celebrity," says M. Bosquillon, "which Dr Cullen has acquired during the more than forty years he has been engaged in the practice of medicine, the great number of pupils who, from all parts of Europe, have visited Edinburgh, in order to profit by his instructions, and the rapidity with which his works have been disseminated, bear ample testimony to his merit. He has successfully combated a great number of deep-rooted prejudices adopted by most physicians, which raises him, in several respects, above the celebrated Boerhaave himself; for the latter, though possessed of astonishing erudition and an exquisite judgment, has been obliged, in collecting all that had been written previously to his own time, to adopt several prevailing errors, from want of sufficient personal experience to enable him to recognise and to combat them.

"The errors of the celebrated men who have preceded him, and the obstacles that presented themselves on all sides, have not deterred Dr Cullen. He has felt that it was

only by reiterated conjectures and experiments that the discovery of truth could be attained. Convinced by the valuable discoveries which we owe to a great number of celebrated physicians, that it was possible to render medicine more generally and more certainly useful than it has yet been, he has, in consequence, attempted a new theory, which, being simple and founded solely upon facts, is preferable to all those that have hitherto been adopted. We recognise in it a man solely occupied with the care of instructing his readers, and placing them beyond the reach of error ; he never proposes his opinions but with the greatest circumspection ; and, far from palliating the defects of his theory, whenever he does not find it clear and evident, he points this out with a good faith which characterises his zeal for truth. But what in a particular manner raises him above those who have preceded him, is the exactness and precision with which he describes each kind of disease, and distinguishes the symptoms that properly belong to it from those which are merely accidental. No author has better pointed out the proximate causes of diseases, but he dwells only on those which are clear and evident ; where this is not the case, he confines himself to facts, of which all his reasonings are merely the consequences. His curative method is always founded on the proximate cause ; he points out with much prudence and judgment the particular powers of medicines ; he explains with clearness their manner of acting, and the cases in which they are suitable, without losing himself in subtile reasonings or in too minute details. Hence, we are indebted to Dr Cullen for an improved theory, joined with much circumspection in the use of this theory, and with great attention to experience and observation, all which qualities render his *Elements of Practical Medicine* a work of the greatest utility."

A new edition of M. Bosquillon's translation was published at Paris so late as 1819 by M. De Lens, and employed by M. Recamier, Professor of Clinical

Medicine to the Faculty of Medicine, as a text-book for his lectures.

In Germany, two editions of a translation of Dr Cullen's First Lines into the German language was published at Leipzig during his lifetime. To the first, in 1778-80, was added an appendix exhibiting his systematic distribution of diseases; and the second, in 1789, was illustrated with notes chiefly derived from the French translation of M. Bosquillon.

In Italy, also, an edition was published, enriched with notes taken from the same source.

It seems not undeserving of remark, that, by means of M. Bosquillon's translation, containing, in the form of notes, large quotations from Dr Cullen's manuscript lectures on the Practice of Physic, and by means of the subsequent translations of these into German and Italian, the medical profession on the Continent of Europe possessed much greater facility of access to many of Dr Cullen's particular opinions than was enjoyed in his own country till the publication of his Works in 1827.

Of the introduction of Dr Cullen's First Lines into the North American States, his warmly attached pupil and friend, the late Dr Rush, gives the following account, in a letter dated Philadelphia, 16th September 1783:—

“ One of the severest taxes paid by our profession during the war, was occasioned by the want of a regular supply of books from Europe, by which means we are eight years behind you in everything. Your First Lines was almost the only new work that was smuggled into the country. Fortunately it fell into my hands. I took the liberty of writing

a preface to it, and of publishing it during the war. The American edition had a rapid sale and a general circulation throughout the United States. It was read with peculiar attention by the physicians and surgeons of our army, and, in a few years, regulated in many things the practice of our hospitals. Thus, Sir, you see you have had a hand in the revolution, by contributing indirectly to save the lives of the officers and soldiers of the American army."

In a letter already given (vol. i. p. 652), which was written about a year after the preceding, Dr Rush says,—“Your First Lines accompany population and government in every part of this Western world. An edition of your last volume is now in the press in this city, and will be published in a few days.”

Among the numerous tributes of respect which Dr Cullen received on the publication of his First Lines, there is one from a quarter whence it could have been little expected, that must have been peculiarly gratifying to his feelings. It is contained in the following passage of a letter (without date) from his former pupil Mr Joseph Ewart, at that time Chargé d’Affaires at the Court of Berlin, to his brother Mr John Ewart, who was then resident in Edinburgh.

“Tell Dr Cullen that all the editions of the First Lines have been translated into German as soon as they appeared, the last not excepted. I ought not to forget to mention, also, that I got a copy of the original from England a few months ago for the great La Grange, the first mathematician of the age, to whom I had given the former edition, and who considers it as the only classical book existing on the Practice of Physic, with the principles of which he is well acquainted, as he is with every other branch of science.”

The distinct and comprehensive definitions and histories of diseases which the First Lines contained,



and the talent which they displayed for the accurate discrimination and simple generalization of the results of experience, were qualities which, in the perusal of the work, could not fail to attract the attention of such a mind as that of La Grange ; and it seems reasonable to presume that it has been those qualities, so difficult to be imitated, which have so long secured for it the highest place among systems of physic. That Dr Cullen's First Lines should now begin to be regarded as, in many respects, unfit to represent the present improved state of the Practice of Physic, is a fate to which every systematic compend of practical knowledge, in a science so progressive in its nature as Medicine, must necessarily be subjected. He seems to have been himself fully aware of this, in the liberal and candid reflections which he makes on the system of his predecessor Dr Boerhaave.

“Whoever,” says he, “will consider the merits of Dr Boerhaave, and can compare his system with that of former writers, must acknowledge that he was very justly esteemed, and gave a system which was at that time deservedly valued. But in the progress of an inquisitive and industrious age, it was not to be expected that any system should last so long as Boerhaave's has done. I apprehend that, in every branch of science with respect to which new facts are daily acquired, and these consequently giving occasion to new reflections which correct the principles formerly adopted, it is necessary, from time to time, to reform and renew the whole system, with all the additions and amendments which it has received and is then capable of. That at present this is requisite with regard to the science of Medicine, will, I believe, readily occur to every person who at all thinks for himself, and is acquainted with the systems which have hitherto prevailed.”

Subsequently to the publication of his *First Lines*, Dr Cullen delivered his lectures on the Practice of Physic in the form of illustrative commentaries on the several paragraphs of that work. The arrangement pursued in both was in conformity with that laid down in his *Synopsis of Nosology*. The full account which has been already given of Dr Cullen's general doctrines in physiology, pathology, and therapeutics, supersedes the necessity of any lengthened exposition of his application of these to special pathology, or the consideration of particular diseases. But the very numerous and often repeated censures which have been passed on particular portions of the doctrines inculcated by him in the *First Lines*, too often by persons who have not had the industry to study his opinions, or have not duly comprehended them, render it proper, if not indispensably necessary, to take a slight review of his published writings and of his oral prelections on the Practice of Physic.

With whatever defects Dr Cullen's general definition of his class *Pyrexiaë* or of his order *Febres* may be conceived to be chargeable, every well-informed and unprejudiced mind will allow, that the history of the phenomena and progress of a febrile paroxysm, as given by him from the sixteenth to the twenty-third paragraphs of his *First Lines*, is, both in respect of the minute fidelity of the delineation and of the clearness and conciseness of the language in which it is portrayed, a perfect masterpiece and model of medical description.

The occasional occurrence in fevers, of congestion or accumulation of blood in some of the internal vis-

cera, to which the attention of the profession has in recent times been particularly directed by Dr Jackson and by Dr Armstrong under the name of Congestive Fever, had not escaped Dr Cullen's observation. In shewing that the appearance of an unusual quantity of bile in the intermittent fevers of warm seasons and climates ought to be regarded as an effect of fever rather than as a cause, Dr Cullen remarks (*Works*, vol. i. p. 505);—"It is obvious that the attack of fever is attended with a constriction of the extreme vessels, which, together with the debility of the body, certainly prevents the blood from being sent in its usual quantity to the extreme vessels. It will therefore be accumulated in the body, and particularly in the venous system, where the circulation is more languid and the dilatation greater. Thus it will be more copiously determined to the vessels of the internal parts, and especially to what I call the hypochondriac system, or system of the vena portarum, in which there is not only the largest proportion of venous blood, but where it is more difficultly transmitted; for wherever the blood is not determined to the surface of the body, we find that, in consequence, it is collected in the vena portarum. And that such congestions are in fact formed in fevers, is well known: the dissections of such as have died of intermittents have shewn a great increase in the bulk of the liver and spleen. But if the blood is thus collected in that region, it must produce a more copious secretion of bile."

The very full account which Dr Cullen gave in his lectures on the Practice of Physic (see *Works*, vol. i. p. 563) of the marks of determination of blood taking place in the progress of fevers to the brain (p. 564), to the lungs (p. 567), and to the abdominal viscera (p. 569), presents so many points of resemblance with the view of the same subject taken by Dr Armstrong in

his Practical Illustrations of Typhus, &c. published in 1816 (p. 25, *et seqq.*), as to render it very difficult for me to believe that Dr Armstrong had not had access to a manuscript copy of Dr Cullen's lectures, or to one or other of the Continental translations of the First Lines, in which copious extracts from these lectures are given.\* This supposition is not the less probable, on account of the affected contempt which Dr Armstrong has elsewhere expressed for Dr Cullen's genius and writings. Dr Armstrong's mode of illustrating the subject of topical determination in fevers, is indeed different from Dr Cullen's in this respect, that while Dr Cullen, in discussing this subject, has given an enumeration of those symptoms only which afford a presumption of the existence of determination to particular parts, Dr Armstrong has given a brief description of the whole succession of symptoms which fever presents, according as it is attended by one or other of these complications; not limiting himself to those which are peculiarly indicative of the local affection. It is certainly proper that the subject should be considered in both ways; but for calling attention very pointedly to the fact of complication, Dr Cullen's method seems to me to be decidedly the better of the two.

In the view which Dr Cullen gave, in his lectures, of the Prognosis in fever, he was accustomed to point out that even in those cases in which symptoms of an extreme degree of debility present themselves, there may be taking place at the same time some topical, and particularly some cerebral, inflammatory affection,

\* See *Elémens de Médecine Pratique de M. Cullen*, traduits par M. Bosquillon, t. i. p. 96.

calculated greatly to aggravate the danger. And, accordingly, he was at much pains to point out those symptoms that, as he says, “express an irritation applied to a certain part of the system, the brain especially, while there are other powers of debility, acting in the same part, and preventing the effects of the irritation from appearing so considerable over the whole sanguiferous system, as if it had occurred as an independent disease;” the circumstances, in short, which express “a combination of irritation and debility, particularly as the irritation occurs in the brain.” (Works, vol. i. p. 589.)

The attempt which Dr Cullen made to ascertain, or rather to promote, the inquiry into the Proximate Cause of Fever, or, in other words, the morbid state or states of the several parts of the body which simultaneously or successively follow the operation of the remote causes of fever, and on which the more remarkable and essential phenomena common to all fevers depend, has given rise to much discussion, and exposed him to much censure at the hands of his critics. From its standing, as it were, at the portal of the First Lines, some persons, dissatisfied with it, seem to have advanced no farther, and to have contracted a general prejudice against Dr Cullen’s Practical Works, from this doctrine or theory. It would not, on any other supposition, be easy to explain how these critics should have been so unscrupulous in applying the name of theorist to a medical author who has seldom if ever been excelled by any systematic writer in the correctness of his descriptions of diseases, and of his practical indications of treatment. Dr Cul-

len's warmest admirers will never claim for his Theory of Fever an infallibility which, as will presently be seen, he never himself pretended that it possessed. But it is due to his reputation shortly to notice the arguments that have been stated in opposition to it, and to endeavour to ascertain what of these are well founded, what of them have originated in misconception, and whether there may not be some that rest entirely on misrepresentation. To facilitate the consideration of the objections which have been urged to this theory, it may be convenient to state in detail the several propositions of which it consists. These, as collected from his writings, seem to be,

*1st*, That there are three states which always take place in fever, a state of Debility, a state of Cold, and a state of Heat; and as these three states regularly and constantly succeed each other, in the order in which they have been mentioned, it is to be presumed that they stand in the relation of cause and effect with respect to one another.

*2d*, That fever chiefly depends upon changes produced in the state of the moving powers of the animal system.

*3d*, That the remote causes of fever (marsh-miasma and human contagion) are sedative powers, which operate by diminishing the energy of the brain.

*4th*, That this diminished cerebral energy is manifested primarily in the animal functions, by the languor, inactivity, and debility of the voluntary motions, by the imperfect exercise of sensation, by a feeling of cold, &c.;—that, in the vital functions, it produces a

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weakened action of the heart and arteries, with atony, accompanied with spasm of the extreme vessels; the effects of which are manifested in the smallness and weakness of the pulse, the paleness and coldness of the extremities, and the shrinking of the whole body;—and that, in the natural functions, this debility or atony of the extreme vessels exists sympathetically in the muscular fibres of the stomach, as is shewn by the occurrence of anorexia, nausea, and vomiting.

*5th*, That the spasm of the extreme vessels is probably not produced directly by the remote causes of fever, but is merely a part of the operation of a general law of the animal economy (the *Vis Medicatrix Naturæ*), whereby it happens that powers which have a tendency to hurt and destroy the system, often excite such motions as are suited to obviate their noxious effects.

*6th*, That this spasm, however induced, proves an irritation to the heart and arteries, and thus gives rise to the phenomena of the hot stage, viz. heat, redness, and turgescence; the spasm, however, still subsisting in it, as is proved by the continued dryness of the surface; and,

*7th*, and last, That the increased action of the heart and arteries thus produced, finally overcomes the spasm, and thereby gives a solution of the disease, the hot stage ceasing with the flowing of sweat and the return of other excretions, which are marks of the relaxation of vessels formerly constricted.

These propositions will be found, it is presumed, to exhibit a concordant, though fuller and somewhat more explicit, development of Dr Cullen's theory of

fever, than that contained in the following summary sketched by himself in the forty-sixth paragraph of his First Lines.

“ Upon the whole, our doctrine of fever is explicitly this : The remote causes are certain sedative powers applied to the nervous system, which, diminishing the energy of the brain, thereby produce a debility in the whole of the functions, and particularly in the action of the extreme vessels. Such, however, is, at the same time, the nature of the animal economy, that this debility proves an indirect stimulus to the sanguiferous system ; whence, by the intervention of the cold stage and spasm connected with it, the action of the heart and larger arteries is increased, and continues so till it has had the effect of restoring the energy of the brain, of extending this energy to the extreme vessels, of restoring, therefore, their action, and thereby especially overcoming the spasm affecting them ; upon the removing of which the excretion of sweat and other marks of the relaxation of the excretories take place.”

The first of the propositions to which I have endeavoured to reduce Dr Cullen's theory, that, namely, which assumes the regular succession, in fever, of the three states of debility, cold, and heat, has been controverted on various grounds. Dr Armstrong alleges (Lecture in Lancet, vol. v. p. 104) that “ Cullen has committed a very serious mistake in supposing that fever, commonly so called, is always attended by the cold stage. He has assumed,” continues Dr A., “ that shivering always attends the commencement of fever ; but I shall shew you that fever frequently has no cold stage at all.” And, in considering fever as capable of arising from three distinct causes, viz. depression, stimulation and irritation, Dr Armstrong expresses his conviction, that, “ when it is produced by the direct

application of stimulants, it is without any cold stage at all."

Dr Cullen was accustomed, in reference to the 34th paragraph of his First Lines, in which allusion is made to "the hot stage of fever being so constantly preceded by a cold stage," to make, in his lectures, the following remark :

"It is at present nearly agreed, that the frequency of the pulse must follow a cold fit, in order to constitute a fever. Sauvages has founded his character of fever entirely upon this opinion ; but it is Dr Hoffmann who has laid the proper foundation of this, and has pointed it out in the most express terms. His words are these, 'For there is no species of it, whether intermittent or continued, mild or malignant, acute or slow, inflammatory, sanguineous, lymphatic or bilious, whether with or without exanthemata, or whether symptomatic, in which there are not observed at the accession, in the course, and even at the time of exacerbation, coldness of the external parts, great constriction of the skin and of its pores, detumescence of the vessels, horripilation, rigor of parts, suppression of sweat, and constipation of the belly.' Here it is asserted in the most positive terms," continues Dr Cullen, "that a fever has constantly as a part of it a cold fit, appearing always once, or occurring more or less frequently." And in commenting upon the definition of the Pyrexiae, he was accustomed to make the following observations : "Neither the *pulsus frequens* of the moderns, which they have made the sole character of the Pyrexiae, nor the *calor major*, which was the sole character of the ancients, is sufficient to constitute a disease of this class ; and they only do so when these symptoms accompany horror, and more or less of debility. Such is the description of fever in every book, and I would not need to have insisted upon it, were it not for an observation of Boerhaave's that the horripilation is confined to diseases from internal causes, while the other symp-

toms are universal ; so that, according to him, there is a hot fit which is not preceded by a cold one. But here to apply a general rule, I say that a fever of this sort does not occur once in ten thousand times. It is possible, that mere violence of exercise and excessive heat may bring on a fever, without any excessive horror ; but even in such cases I have always observed that the horror did take place before any *permanent* fever came on ; and if it does not, in no case does the fever come on, unless there is some topical affection, some congestion, which is the cause of inflammation. And however we may speak in common language with respect to our being in a fever, upon many occasions this has been owing to the mistake of physicians with respect to its nature ; and the character must remain as it stands, that the horror is universally the prelude to all kinds of fever."

According to Dr Armstrong (loc. cit.), Dr Cullen has committed another mistake by asserting that the cold stage is always followed by the stage of reaction or excitement, the ground of his contradiction seemingly being, that common congestive fever sometimes continues as a distinct variety, and may be resolved entirely by the means used, without reaction supervening. That the cold stage of fever may terminate in death, without any reaction supervening, and this at very various periods of the disease, was well known to Dr Cullen. But that the cold stage may terminate in resolution or restoration to health, without reaction occurring, is an opinion which will not readily be recognised as true by those who have attended to the course of fevers. Such a form of disease, indeed, if it occurred, might constitute a *congestion*, but certainly would not constitute a *fever*, according to any interpretation of this term that has ever been recognised by medical men.

The second proposition, that fever chiefly depends upon changes in the state of the moving powers of the animal system, which Dr Cullen adopted principally from Hoffmann, has been generally recognised as true by medical inquirers since his time.

M. Pinel, after quoting Hoffmann's statement of the succession of phenomena that occur in fevers, puts the question, "Has not Cullen expounded the same principles? and how could he appropriate to himself this doctrine respecting the proximate cause of fever, without recalling the source from whence he had derived it?" Nothing seems to have been more remote from Dr Cullen's nature than a disposition to appropriate to himself the opinions of others. In the paragraph of his First Lines in which he describes the share which he supposes spasm to have in producing the phenomena of fever, he subjoins a reference to the passage in which Hoffman had advanced that doctrine; and in various passages of his lectures, he explicitly stated what parts of his theory of fever were derived from Hoffmann, and what part he considered to have originated with himself.

The third proposition, viz. that the primary effect of the remote causes of fever is a diminished energy of the brain, occasioning a debility of the whole of the functions, was, indeed, the only part of the theory of fever stated by Dr Cullen, to which he laid claim as his own.

"Dr Hoffmann's system," says he, "goes no farther than the spasm and hot fit, not taking in debility at all; and I own that I have assumed the existence of this state of debility, previous to the spasm, before it has been clearly proved to exist in every case." "It is surprising," he adds, "that physicians have so long overlooked debility as a considerable

part of the proximate cause of fevers. Nobody can miss to observe that in the course of fevers many symptoms of debility appear, and go on constantly increasing with the disease when it proves fatal."

The soundness of Dr Cullen's general view upon this subject seems to be commonly acknowledged. "That fever in its commencement or earliest stage," says Dr Good, "is characterised by debility of the living fibre, or, more closely in the words of Dr Cullen, by diminished energy of the brain, extending directly or indirectly to the voluntary muscles and capillaries, cannot for a moment be doubted by any one who accurately watches its phenomena; and thus far the Cullenian hypothesis is unquestionably correct." "The first position of this celebrated doctrine," says Dr Joseph Brown of Sunderland, "the work of a man whose powerful mind has still left its impress on our [medical] reasonings, and perhaps on the practice of some of us, viz. that the energy of the brain is diminished, is a fair inference from the phenomena of almost every case of idiopathic fever."

The share, however, which Dr Cullen conceived Debility to have in producing and determining the phenomena of the cold stage of fever, as indicated in the 4<sup>th</sup> proposition, has most unaccountably been misrepresented by several authors. Dr Southwood Smith, in professing to point out the errors common to all the theorists on the proximate cause of fever, has made a statement, seemingly intended to refer to Dr Cullen, among others, but which, in so far as he is concerned, is assuredly singularly misapplied. "The believers in debility," says Dr Smith, "derive their notion of the whole disease from the phenomena which



occur in the first and the last stages only : in these, it is true, they may find abundant evidence of debility ; but then they overlook the intermediate stage in which there are generally the most unequivocal indications of increased sensibility in the nervous, and increased action in the vascular, systems. In this manner," he adds, " they characterise the disease by what appertains only to certain stages of it."

Dr Cullen's summary of his Theory of Fever, above quoted (p. 113), and which is quoted by Dr Smith himself, bearing, as it does, that, by the intervention of the cold stage, &c., the action of the heart and larger arteries is increased, and continues so till it has had the effect of restoring the energy of the brain, &c., sufficiently proves that he at least cannot be charged with having overlooked the intermediate stage of increased action, either as regards the vascular system, or those other functions which he conceived to be dependent on the energy of the brain. And the account which Dr Cullen has himself given, from § 127 to § 200 of his First Lines, of the means to be employed for moderating the violence of reaction in fevers, might, one would reasonably have supposed, have prevented the possibility of his ever having been charged with overlooking " that intermediate stage of fevers in which there are generally the most unequivocal indications of increased sensibility in the nervous, and increased action in the vascular, systems."

Dr Smith says that " Brown, like his predecessor, attributes all fevers to debility." But he might have learned from Dr Currie's Reports (5th edit. vol. i. p. 234), or a little consideration might have enabled him to perceive, that between the doctrine of debility in fever taught by Dr Cullen, and that taught

by Brown, there was this wide and essential difference, that whilst debility was only the first link in the chain of Dr Cullen's theory, according to Dr Brown's, it formed the sole and continued essence of fever. It is not a little amusing to find, that when Dr Smith, after much pretension to the observance of a more rigid philosophic method, in tracing the succession of phenomena in fever, comes to expound his own theory of this state, the account he gives of the first change produced in the economy by the operation of the exciting causes, proves to be nothing more than a paraphrase of the corresponding part of the doctrine of Cullen.

"In conclusion, then," says he, "the doctrine of fever which appears to approximate most nearly to the truth, may be summed up in few words. The immediate cause of fever is a poison, which operates primarily and specifically upon the brain and the spinal cord. The diseased state into which these organs are brought by the operation of this poison, deprives them of the power of communicating to the system that supply of stimulus (nervous and sensorial influence), which is requisite to maintain the functions of the economy in the state of health. The organs, the seats of the functions, deprived of their supply of nervous influence, become deranged, the derangement in each taking place in a fixed order, and in a determinate manner."

The author of the article Fever, in the *Cyclopædia of Practical Medicine*, has also made some remarks in reference to Dr Cullen's doctrine of debility in fever, of a vague and inaccurate character. After stating that "Cullen believed that in fever certain causes produced collapse or diminution of the energy of the brain, the effect of which, on the voluntary muscles and extreme vessels, was universal debility, &c." he adds, "The promi-

nent importance the Edinburgh Professor assigned to the fictitious debility which was imagined to result from this unknown condition of the brain, notwithstanding his theory that there was an inherent protective power in the system by which this fancied weakness was to be overcome, has been followed by serious practical errors, by abstracting the young and inexperienced mind from the more acute forms of fever, and from those important local complications which very frequently take place in its progress."

By the *certain causes* alluded to in the above passage, we are, it must be presumed, to understand the remote causes of fever. Why the diminished energy of the brain, arising from their operation, should be said to be an *unknown* condition, is not very obvious; it can be known only, like every thing else in nature, by the effects which it produces, or its external manifestations. The terms "fictitious debility" and "fancied weakness," which states Dr Cullen is said to have *imagined* to result from this unknown condition of the brain, seem to imply a disbelief in the actual occurrence of such a state, though the phenomena attributed to that debility, might have been thought to leave little room for scepticism on this point. That Dr Cullen's theory of fever has any tendency to abstract the young and inexperienced mind from the more acute forms of fever, and from those important local complications which very frequently take place in its progress, is an opinion which we cannot very readily admit, seeing that no practical author with whose writings we are acquainted, has been at more pains to point out the symptoms which denote the violence of reaction as occurring in fever, particularly those which are the marks of a general inflammatory

diathesis, and more especially of particular local determinations to the brain, to the lungs, and to the abdominal viscera, such as frequently occur in the low nervous or typhus fever.

Dr Cullen's opinions respecting the supervention of Spasm in the cold stage of fever, its cause, the effects to which it gives rise, and its mode of termination, as stated in the 4th, 5th, 6th, and 7th propositions have been the subjects of much animadversion, and of no small share of misconception and misrepresentation.

“ When Cullen goes on to affirm,” says Dr Smith, “ that the proximate cause of all the morbid phenomena (of fever) is a spasm of the extreme vessels, he commits the additional and more palpable, but not less common error, of assigning as an undoubted fact, as a real and ascertained occurrence, what is only a conjecture, and for which there is not, and for which he does not even attempt to adduce, the shadow of evidence.”

In the first place, Dr Smith has no grounds for the assertion that Dr Cullen affirmed spasm of the extreme vessels to be the proximate cause of all the morbid phenomena of fever. Dr Cullen's theory of fever did not admit of the supposition—as its opponents seem uniformly to imagine—that all the morbid phenomena of that affection depend on any one proximate cause whatsoever. A debility of the nervous power, as forming the beginning of the cold fit, he considered to lay the foundation of all the other phenomena (Works, vol. i. p. 492); and from the diminished energy of the brain, he endeavoured to deduce a succession of internal pathological conditions, as giving rise to the various phenomena that manifest themselves in the progress of fever.

He thought that spasm of the extreme vessels might be considered, not, as Dr Smith says, as the proximate cause of all the morbid phenomena, but as a principal part in the proximate cause of fever (§ 41); and in the same way he conceived that an atony, subsisting in the extreme vessels on the surface of the body, may be considered as a principal circumstance in the proximate cause of fever (§ 44); but he remarks, “this atony we suppose to depend upon a diminution of the energy of the brain” (§ 45). But, in the second place, it is not easy to understand how Dr Smith should have alleged that Dr Cullen does not even attempt to adduce the shadow of evidence for the occurrence of a spasm of the extreme vessels in fever. The fortieth paragraph of the First Lines contains a distinct,—whether or not it shall be considered a satisfactory,—statement of his reasons for recognising the existence of such a morbid condition.

“It is to be particularly observed,” Dr Cullen remarks, “that during the cold stage of fever, there seems to be a spasm induced everywhere on the extremities of the arteries, and more especially of those upon the surface of the body. This appears from the suppression of all excretions, and from the shrinking of the external parts; and though this may perhaps be imputed in part to the weaker action of the heart in propelling the blood into the extreme vessels, yet as these symptoms often continue after the action of the heart is restored, there is reason to believe that a spasmodic constriction has taken place; that it subsists for some time, and supports the hot stage,—for this stage ceases with the flowing of the sweat and the return of other excretions, which are marks of the relaxation of vessels formerly constricted.”

It is obviously open to inquiry, whether the pheno-

mena that were attributed by Hoffmann and Cullen to spasm of the extreme vessels, may not be more satisfactorily accounted for on some other supposition than that which they adopted. By Bellini and Boerhaave, as Dr Cullen was accustomed to observe in his lectures, "the symptoms of the cold fit were supposed to be produced by a viscosity of the blood, occasioning its stagnation in the extreme vessels; and this," adds Dr Cullen, "has continued to be the doctrine of all the Boerhaavians till lately; and M. Quesnay, in particular, has adopted it." More recently, Dr Joseph Brown conceives that the phenomena of the cold stage of fever admit of being explained independently of the supposition of spasm of the extreme vessels. "The diminished energy of the sensorium," says he, "lessens the action of the heart; whence the paleness and the coldness of the surface, which require not the intervention of this assumed spasm to explain them, but arise, whether at the commencement of fever or in the cold fit of ague, from want of nervous energy and of the due propulsion of blood to the capillaries." But it is obvious from the statement in the fortieth paragraph of the First Lines (above quoted), that it was not so much the phenomena of the cold stage of fever, considered in themselves, but as viewed in connection with the phenomena of the hot stage, that induced Dr Cullen to adopt the supposition of spasm of the extreme vessels. And it may be doubted whether any other hypothesis has hitherto been proposed, that enables us to conceive the manner in which the perspiration may continue to be suppressed, often long after the temperature of the

body has been restored and the vigour of the capillary cutaneous circulation has become morbidly increased.

Dr Parr has remarked, in speaking of Dr Cullen's theory of fever, "that in this system the production of a spasm by debility is an isolated fact, without a support; and the introduction of the *vis medicatrix naturæ* is the interposition of a divinity in an epic, when no probable resource is at hand;" and this observation has been quoted again and again, as if of oracular authority. Dr Mackintosh, in his Elements of the Practice of Physic, says,—“It is remarkable that Cullen, who has insisted with so much pertinacity on spasm of the extreme vessels being a principal part of fever, should so completely have forgotten himself as to assert that atony, which is the very reverse of spasm, is also a principal circumstance in the pathology of fever.”—“The meaning of spasm of the extreme vessels,” continues Dr Mackintosh, “is morbid contraction; that of atony of the extreme vessels is a defect of muscular contraction. Can a morbid contraction and a morbid relaxation coexist in the same vessels in the same disease?”

In the account that has been given of Dr Cullen's lectures on physiology and on general pathology, his illustrations of the influence of causes of debility in producing convulsions, or of acting as, what he termed, indirect stimulants to the muscular system, have been already noticed. (Vol. i. pp. 302 and 382.) These sufficiently establish the frequent coexistence of debility of the general system with irregularly increased motions of the muscular system, in whatever manner this singular conjunction is to be explained. His observations on tremor and palpitation (p. 387) are con-

firmatory of the same view. In reference to the difficulty adverted to by Dr Mackintosh,—the occurrence of spasmodic contraction in vessels supposed to be in a state of debility,—Dr Cullen was accustomed to observe :—

“ It may seem difficult to understand how an atony and spasm subsist at the same time ; but whatever difficulty there may be in accounting for this, we consider it as a matter of fact, and at the same time think it may be found analogous to what happens upon other occasions in the system, where we often observe atony producing spasm. The symptoms of *debility* are evident, as are also the marks of the *spasm*, and we know that weak and irritable systems are most liable to the several spasmodic affections.”

It is not a little singular that Dr Parr, after declaring the production of spasm by debility to be an isolated fact without a support, should, in the very next column of his Dictionary, have stated that, under the article “ Convulsions,” and in other places, he has endeavoured to shew that either real debility or unequal excitement is the cause of irregular action of the muscles, and this, he says, appears in the tremors and rigors, *and in the spasm of the Capillaries*, not only of the surface and glands, but of every other organ.

The mode in which Dr Cullen, in following Hoffmann, has attempted to account for the increased action of the heart and arteries, which takes place in the hot stage of fever, and to establish that the cold stage is the cause of the hot stage, has been the subject of much animadversion, originating in a great degree in misconceptions and misrepresentations as to the opinions he entertained on the subject of the *Vis Mediatric Naturæ*. (See Appendix G.) Dr Good urges it



as “ a striking defect in Dr Cullen’s Theory of Fever, that must occur to the attentive reader, that debility is here made a cause of strength, the weakened action of the first stage giving rise to the increased action and re-excited energy that restore the system to a balance of health ; and here again,” says he, “ we stand in need of the interposition of some present divinity to accomplish such an effort by such means.” Dr Good, however, is disposed to consider the Cullenian hypothesis correct “ in supposing the cold stage to be the foundation of the hot,” but it must surely be as difficult to explain how a cold state of the body shall lay the foundation of a hot state, without the interposition of some present divinity, as how weakened action shall give rise to increased action.

“ I have little doubt,” says Dr Cullen, “ that repeated paroxysms of fever may induce debility, in consequence of the violent excitement ; but debility occurs in the *very beginning* of the disease also, in a very strong degree ; and, however difficult we may find it to explain how debility produces reaction, yet we find that the one precedes and the other follows, and, we presume, from the order of their succession, that they are connected as cause and effect.” (Works, vol. i. p. 573.)

“ Agreeably to the opinion of most part of physicians,” he observes in the 59th paragraph, “ I suppose that in every fever there is a power applied to the body which has a tendency to hurt and destroy it, and which produces in it certain motions which deviate from the natural state ; and, at the same time, in every fever which has its full course, I suppose that, in consequence of the constitution of the animal economy, there are certain motions excited which have a tendency to obviate the effects of the noxious powers, or to correct and to remove them. Both these kinds of motion are considered

as constituting the disease. But the former is, perhaps, strictly the morbid state, while the latter is to be considered as the operation of the *vis medicatrix naturæ*, of salutary tendency, and which I shall hereafter call the REACTION of the system."

"In what manner the state of debility brings on, with respect to the heart and arteries, a seemingly contrary state of force and activity, is a difficulty which we do not pretend entirely to remove. We can only refer it to a general law of the system, which, however, is very well established, and is shortly this: That where a deviation from the natural state of health happens, from the nature of the economy this deviation naturally produces a tendency in the system to restore itself to its former condition. This, I say, constitutes the *vis preservatrix* and *medicatrix naturæ*."

And after quoting the 120th paragraph of his Physiology, he proceeds:

"For further information with respect to this law, see Dr Gaubius's Pathology, from par. 633 to 650, where you will find commentary enough upon what I have read. There you will find that, while many physicians refer this entirely to the mechanism of the body, thinking that it is merely the effect of one corporeal part acting upon another, or that the one state necessarily follows the other from the laws of animal mechanism, their succession depending entirely upon mechanical principles, others think that the mechanism of the body is by no means equal to such effects, and refer it to the powers of the mind, considering it as an operation of the rational soul. Dr Gaubius himself takes a middle course, making it in part to depend upon the mechanism of the body, but also in a great measure upon the operation of the mind, as the matter was explained by Dr Stahl, acting separately and independently of the body, and directing the motions of it without any necessary and mechanical measures. Now, with regard to this, I assume the existence of

a *vis medicatrix*, as a fact, and, to our present purpose, I particularly assume the following instance of it, that when any power has been applied to weaken the action of the nervous power, the debility thereby occasioned, when not carried too far, produces a reaction of the brain, or an increase of its energy; and I have laid a foundation for this in the paragraph of my Physiology following that which I have read.

“ Now, I say, if it shall be proved that, in so many instances of cases of debility, a reaction ensues, the analogy may fairly be transferred to the case in hand, and it may be referred to a general law of the system. But it is desirable to go a step further; and here I have constantly rejected the Stahlian system, and I do not compound the matter as Gaubius does, but assert that the mechanism of the body is particularly concerned. Now to take into consideration the mechanism of the body, in this particular case, will be difficult. But the case is attended with peculiar circumstances; and if any of them can throw any light upon this matter, they are such essential parts of fever as require our particular consideration.”

Such is an outline of the general doctrine of Fever, as taught by Dr Cullen; and a view of some of the principal objections which have been urged against it by those who, like him, have been disposed to recognise the existence of idiopathic or essential fevers,—that is, of fevers which, whatever accidental complications they may experience in their progress, are not necessarily, in their origin, accompanied with, and, at all events, are not preceded by, nor dependent upon, any primary local affection; in which, to use a convenient term, the fever is protopathic, not deuteropathic.

It would be foreign to our present object to enter on the consideration of some recent pathological doctrines, according to which no such disease as an idiopathic or

protopathic fever is recognised, all fevers being supposed to originate out of some primary local inflammation. But it may be remarked, that even assuming that these views were established as correct, or that, in the advancing state of pathological chemistry, it were ascertained that the primary operation of the exciting causes of fever is upon the blood, the ascertainment of such a fact would only prefix another link to the chain of morbid conditions or proximate causes recognised by Dr Cullen as producing the phenomena of fever; as he, in the recognition of the debility of the nervous system, prefixed an additional link to the chain recognised by Dr Hoffmann. It would merely establish that the production of this debility is not effected directly by the operation of the exciting causes of fever on the nervous system, but through the medium of some previous change which those powers produce in some other part of the economy. So far, therefore, as Dr Cullen's theory can be considered a correct generalization of facts, it must remain in full integrity amidst any such extensions of medical knowledge.

M. Pinel was one of those who joined in the censure of Dr Cullen for the large share of attention which he gave to the investigation of the proximate causes of diseases. "Cullen, a celebrated physician of Edinburgh," says he, in an article on the Method of Study in Medicine, prefixed to his Philosophical Nosography, "acknowledged having imbibed the first principles of medicine in the writings of Boerhaave; but in the preface to his work, he gives a judicious criticism of these writings, and exposes with great judgment their imperfections and defects. In the course of his work, however, he engages himself in other vain subtleties, and supposes himself able fully to unveil the

mode of production and the proximate cause of fevers, of inflammations, and of other diseases, in place of confining himself simply to describing their history."

The contradictory complaints that have been urged against Dr Cullen, of having, in his Nosology, paid too much attention to the symptoms of diseases and too little to the internal conditions which produce them ; and of having, in his lectures and writings on the Practice of Physic, wasted his own time and that of his pupils in searching after proximate causes, seem to proceed from inattention to the purposes which, on each occasion, he had it more especially in view to promote ; that whilst, in the Nosology, it was his aim to establish the means of diagnosis, his principal concern, in his practical writings, was to establish the indications of cure. That it is truly on the symptoms of diseases that the medical practitioner must depend for his diagnosis, and that it is on his notions of the internal morbid conditions or proximate causes of diseases that he should endeavour to found his method of treatment, will scarcely be denied. Dr Cullen's example might, at all events, satisfy the opponents of symptomatical nosology, that there is no inconsistency between a minute attention to the symptoms of diseases, and a zealous attempt to arrive at a knowledge of those internal conditions of which the symptoms are only the outward manifestations, and which internal conditions, they so strenuously insist, ought to be considered as constituting the real disease.

In considering the merits of Dr Cullen's theory of Fever,—or, indeed, of any other part of his medical writings,—it must always be kept in view, that the

great aim of all his physiological and pathological investigations was to ascertain the established order of succession in the various trains of healthy and diseased phenomena which occur in the animal economy. Adopting the views of his friend Mr Hume on Causation, he has assumed as a principle to guide him in his reasonings, that any phenomenon or event which immediately and invariably precedes another, is to be considered as the cause, and that event which uniformly follows, as the effect. Various unknown changes may indeed intervene in the animal economy between events that seem to us to be related as causes and effects, but our ignorance of these intervening links does not prevent us in medicine, any more than in any of the other branches of physical science, from regarding the invariable apparent antecedent of any event as the cause of that event. In the explanations which Dr Cullen has given of the phenomena of the animal economy, and of his proximate causes or theories of diseases, the chief object of his investigations, therefore, was the discovery and simple expression of the more regular and uniform order in which these phenomena succeed one another in nature, and not a vain pretension on his part to explain the production of one phenomenon merely by the occurrence or presence of another. He has not, as the observations of some physicians would lead us to believe, attempted to describe what he conceived might or ought to be the order of nature in the phenomena of fever, and of the other morbid processes of which the human economy is the seat ; but his great and pre-eminent aim evidently was merely to detect what that order really

is. "My business is not," he himself observes, "so much to explain how this and that happens, as to examine what is truly matter of fact." (Works, vol. i. p. 442.) "My anxiety is not so much to find out *how* it happens as to find out *what* happens."

Accordingly, in his lectures on the Practice of Physic, after having explained the opinions which he entertained respecting the nature and proximate causes of fever, and detailed the particular facts on which these opinions were founded, he was accustomed to remark :—"I have now given you my general scheme of the Theory of Fever, and it amounts to this ; That in every fever, properly so called, there occur these three states, of debility, spasm, and increased action of the heart and arteries ; and as these succeed to one another in the order just now mentioned, upon the ordinary grounds of reasoning we conclude that they are so far cause and effect. This, then, is what I call the Theory of Fever ; but observe what sort of theory it is ; it amounts to this—the establishing of certain facts, or that certain states do take place successively in the system. Here is no mysterious doctrine of the nervous system ; I propose a nervous power, indeed, which every body supposes to exist, as well as they suppose the existence of their eyes, but our theory is the same whatever opinion we hold with respect to the nature of that power. With regard to the mutual connection of these several states, I have only gone so far as to say that it has a reference to a general law of the system,—or that there is a *vis medicatrix naturæ*, or *autocrateia*,—or that the system has a power to redress its own deviations,—or that there is a power in the economy to resist and remove such things as are hurtful. So this I suppose to form the connection between the several states of fever, which, indeed, every system supposes, viz. that there is an effort of nature

to remove something that is noxious, and which, by its continuance, would disturb the system or destroy it.

“ This theory of fever amounts to no more than saying that there are certain states of the body which are combined together in a certain order of succession, and that, from this constant combination, they are to be considered as a series of causes and effects. This I assume as a fact, and I suppose that this connection is determined by a certain mechanism or organization of the body; but I do not pretend to explain the nature of this. I might offer conjectures upon this subject; but while they are only conjectures, I shall not trouble you with them, so shall go no farther. But the length we have gone, I hope we shall find it applicable to the practice, for I have only referred some part of the connection to a certain general law of the system, to the *vis medicatrix naturæ* or *autocra-teia*, which, though it does not explain the connection, assists in establishing it.”

That to trace the order in which the phenomena of the animal economy succeed to one another, and, from this succession of external phenomena, to attempt to infer, what is the succession of internal pathological conditions,—or, in the language of some pathologists, to arrive at the *ratio symptomatum*—in each form of disease, was the object which Dr Cullen kept constantly in view in all his observations and speculations, and of which he proposed to exhibit an example in the composition of his First Lines of the Practice of Physic, may be presumed, not only from the rules which he laid down for himself, but also from the manner in which every part of that work has been executed. His main design in this respect, however, seems to have been but little if at all perceived or understood by many of his critics, who



have so often regarded and represented the generalizations to which he was led from observation, reading, and reflection, as merely hypothetical assumptions. Every re-perusal of Dr Cullen's First Lines of the Practice of Physic, and comparison of them with the other medical text-books at that time in existence, or which have since appeared, tends to confirm me in the belief that his work exhibits in the execution of all its parts, but particularly of that relating to fever, the most skilful and successful application of the principles of inductive philosophy, to the study and practice of physic, that has ever been made by any single physician; and notwithstanding all the advances which have since been made in the auxiliary and collateral branches of medical science, and particularly of pathological anatomy, I doubt whether it has yet been, or is likely soon to be, surpassed or equalled.

“ It is not Dr Cullen's praise,” as has been well observed by Dr Wilson Philip, “ that he invented an ingenious hypothesis, but that he new-modelled, if I may use the expression, the whole practice of medicine; that, in his *Synopsis Nosologiæ Methodicæ*, he defined and arranged diseases with an accuracy that is still unrivalled; and, in his *First Lines*, reduced their treatment to a degree of simplicity formerly unknown.”

The view which Dr Cullen took of the Remote Causes of Fever, must be acknowledged, even by those who may be disposed to question its accuracy, to have presented this subject under a much more simple aspect, and one more calculated for the elucidation of truth, than any in which it had ever previously been exhibited; and if its details cannot be considered as having originated with him, he is certainly

entitled to the credit of having put them together in such a way as to constitute a general doctrine, capable of improvement according to the progress of knowledge.

Dr Cullen conceived that the remote causes of fever are chiefly of two kinds, viz. 1st, Effluvia from the Human body ; and, 2d, Effluvia from other substances, and particularly from Marshes. The effluvia of the former description he regarded as the principal cause of Continued, and those of the latter as the principal, if not the only, cause of Intermittent fevers ; and he conceived that, for general purposes, these two kinds of effluvia may be distinguished from one another by the names of Contagions and Miasmata. He was perfectly aware of the fact which has been adverted to by Dr Good, that the terms Miasma and Contagion do not, etymologically considered, justify the signification which he has attached to them ; but the accurate definitions he has given of the sense in which he proposes to employ these terms, in the 78th and 84th paragraphs of his First Lines, and which a very slight modification would render perhaps unobjectionable, make this a matter of little importance. Dr Good says, that “ no great benefit has resulted from endeavouring to draw a line of distinction between these two terms, and that hence it is a distinction which has been very little attended to of late years.” It may be questioned, however, whether the reverse of Dr Good’s proposition would not more correctly express the fact, that the little attention which has been paid to this distinction, has prevented the benefit which would otherwise have arisen from its observance.

The very curious and important investigation into the source and natural history of the Agent which produces Intermittent and Remittent Fevers,—commenced by Lancisi in Italy, and followed up by the excellent and accurate observations of Sir John Pringle, and Dr Lind,—the three authorities upon which, conjointly with his own experience, Dr Cullen chiefly relied in his speculations upon this subject,\*—has been continued to the present day by a succession of zealous and intelligent medical philosophers and practitioners in all the different regions of the globe. Many efforts have been made to demonstrate the physical existence, and to ascertain the chemical nature of this agent, the precise conditions of its origin, the laws of its dispersion through the atmosphere, and particularly the distance to which it may be conveyed, by atmospheric currents, from the seat of its production.† But though intermittent and remittent fevers have been observed to occur chiefly in the neighbourhood of marshes, and the name of marsh poison has, from this circumstance, been generally applied to the agent to which their production is attributable, it is conceivable that the combination of conditions essential to its development, including peculiarities of soil, moisture, and heat, may occur in situations that cannot be strictly designated as marshes; and that intermittent and remittent fevers may consequently arise

\* Torti, Werlhoff, and Cleghorn, were Dr Cullen's chief authorities on the varieties of Intermittent Fevers.

† See particularly the writings of Sir Gilbert Blane, Monfalcon, and Dr Macculloch.

in such situations;\* and it is conceivable, also, that in situations which usually fall under the designation of marshes, the necessary combination of conditions may be wanting, so that the morbid agent shall not arise, and consequently intermittent and remittent fevers shall not be developed. On the whole, it seems probable that this morbid agent is always present and active in warm climates; but that in the temperate and colder, its generation being dependent on the simultaneous and alternate action of heat and moisture, it is chiefly at the stated periods of spring and autumn that its operation is rendered most manifest. For an excellent summary of the results of the observations and experiments that have been made on this important subject, I have much pleasure in referring to the chapter on Ague in the Elements of the Practice of Physic by my learned and intelligent friend Dr Craigie.

It is well known that in warm climates, not only are intermittent and remittent fevers convertible into one another, but they not unfrequently pass into fevers of a continued type; whilst those of a continued type pass, though perhaps less frequently, into intermittent and remittent fevers. It is equally well known that, in the British islands, and in every other country of Europe, there exist fevers of a continued type, which occur sporadically, and even sometimes prevail epidemically, without there being any reason to believe that they have had their origin in human effluvia, or that

\* See the interesting observations of Dr W. Ferguson and Dr Wilson.

they are, in ordinary circumstances, liable to be, if indeed they are capable of being, directly communicated from one individual to another. It is now tolerably well ascertained, also, that the contagious fever of these islands, termed Typhus, loses its infectious character or quality in being conveyed to warm climates; and, consequently, that patients affected with it may, when carried to these climates, be safely placed in private houses or in hospitals, without any danger of infection being communicated to those with whom they are brought into intercourse,—a fact which I first learned from a near connection and pupil of my own, the late Dr William Gordon of Berbice, and which has since been confirmed to me by the writings of Dr Jackson, and by conversations with him and with several other West India practitioners.

It was the knowledge of these facts, together with my having myself repeatedly observed in this country, and also over a large tract of the continent of Europe in the summer and autumn of 1814, continued fevers of an uncontagious character, that led me, in commencing to teach the Practice of Physic in 1822, to lay aside the division of Continued fevers which had been adopted by Dr Cullen, and to propose one which I regarded as more simple and conformable to the present state of our knowledge. In adopting Synochus as a generic term for Continued Fever, as suggested by him, I divided it, on etiological grounds, into the two species of Non-Contagious and Contagious Synochus, each of which may be considered as comprehending three Diathetical forms or varieties, according as the predominant character of the disease shall appear

to be inflammatory, nervous or typhous, or putrescent or bilious. These diathetical forms, indeed, are observable in Idiopathic fevers of all types—intermittent and remittent, as well as continued; and they are equally observable in all the orders of Symptomatic pyrexiae, or the fevers accompanying Inflammations and Hæmorrhages, and particularly the Exanthemata and Profluvia.

Dr Cullen, however, was not disposed to exclude altogether from the character of remote causes of fevers, some other agents besides human and marsh effluvia, particularly the power of cold applied to the human body, fear, intemperance, &c. (§ 87, seq. 9.) “I will not,” says he, in his lectures, “positively determine that cold may not of itself produce fever, but it very seldom does so; and I am persuaded that if it ever produces any thing like fever, it is the simple and mild Synocha.” But, both in respect of cold and of the other supposed remote causes of fevers that have been referred to, he was satisfied that their influence in the production of these diseases is chiefly exerted in concurrence with the operation of marsh or of human effluvia.

Physicians, as Dr Cullen was accustomed to point out, had been greatly divided with respect to the question, how much ought to be attributed in the production of Epidemics, and particularly of epidemic fevers, to the sensible or physical qualities of the atmosphere, comprehending its moisture, temperature, electricity, &c., and how much to its insensible qualities, meaning thereby its impregnation with marsh and human effluvia; and whether these two sets of qualities are to be

considered as acting separately or in conjunction. He was not himself disposed to admit, on the one hand, that the sensible qualities of the air are the sole agents on which the production of epidemic fevers depends, nor, on the other hand, that these qualities do not exert any influence upon such epidemics. While he believed miasmata and contagions to be the chief causes of fever, he acknowledged that the sensible qualities of the air, its degrees of temperature and moisture, &c. may exert an influence over those, its insensible qualities, and may modify the character of the epidemic which they produce. Thus heat, particularly as combined with moisture, he considered to be a leading circumstance in the production of marsh effluvia; the tendency of epidemics to a putrid (including a bilious) or to an inflammatory character, he conceived to be referrible to the degree of heat or coldness of the atmosphere; and he was satisfied that the same qualities have also an influence upon the type which fever assumes. (Works, vol. i. p. 558.)

We have seen that, whilst Dr Cullen regarded Synocha as attributable to cold, he considered both Typhus and Synochus to be contagious diseases, or rather to be varieties of the same disease arising from the same cause, though differing in their diathetical character. But, independently of those epidemic continued fevers which propagate themselves by contagion, agreeably to Dr Cullen's definition of that term, and of those which, as exhibiting more or less of an inflammatory complication, may be considered as Synocha produced by cold—Epidemic fevers of a continued type unquestionably occur in temperate as

well as in warm climates, in which there is no room for suspecting human contagion to operate, and in which, at the same time, there is no obvious condition of the sensible qualities of the atmosphere to which they can be attributed. As to the nature of the remote cause or causes on which such epidemics depend, whether it consist in a mixture, with the atmosphere, of a terrestrial emanation different, in its essence or in its accidental or adventitious qualities, from that which produces intermittent and remittent fevers, or whether it be some undiscovered modification of the physical qualities of the atmosphere itself, there obviously still remains much room for speculation and inquiry.

In ascribing so large a share of influence, in the production of fevers, to the operation of marsh and human effluvia, Dr Cullen was farther led to the belief, that in respect of each of these kinds of effluvia, there exists but very little variety.

“ A very important object of our practice,” says he, in recapitulating, in his lectures, his opinions relative to marsh miasmata and human effluvia, “ is the preservation of men from fevers, which must chiefly turn upon our knowledge of the remote causes. Instead of following the common opinion, I have endeavoured to seek for causes more general than those which have been usually described, and now that it is brought forth, it may be wondered that it has not been perceived before. It is agreed among physicians that fevers depend upon miasmata and contagions ; but the endless variety that appeared at first prevented them from inquiring into the matter ; we have, therefore endeavoured to obtain a more simple view of it. Nothing is better established than that there are certain exhalations and effluvia arising from marshy grounds in hot seasons which are the cause of diseases, and particularly of intermit-



tent fevers. And, again, it is certain that we know of no intermittent fevers being epidemic, but where we can trace their source to such exhalations. The sameness of the different kinds of intermittent fevers leads us to believe that the nature of these exhalations is identical, that they are nearly of one kind, and that, therefore, we have hit upon the most general, nay probably the only, remote cause of Intermittent and Remittent fevers.

“ As to Contagions, they are of two kinds, one that seems to be constantly subsisting in the world, and ready to appear whenever the circumstances of the season are favourable to its activity ; and as we have now, by the experience of some hundred years, found that certain contagions produce always the same diseases, we consider these as *specific* contagions. They may be at one time latent and dormant, and at other times active ; but from their producing the like diseases in different periods and seasons, there must be something we call specific in them. The origin of these contagions in particular places is very uncertain, as also who have been the first writers respecting them ; but it would appear that they are never generated anew. Another kind of contagions is such as may, in any part of the earth, under certain circumstances be generated, and again, by other circumstances, be destroyed or extinguished. These we call *common* contagions. Now, the variety of epidemics will depend upon the variety of these specific and common contagions.”

With regard to Common Contagions, Dr Cullen observes ; “ If we can find distinctly but two sources of fevers, viz. marsh miasmata and human effluvia, and if, in fact, besides the singularity of the diseases arising from them, we can trace the most part of epidemics to one or other of these sources, and if every day shews the greater similarity between the diseases of different years and countries, we must conclude that common contagions are fundamentally of little variety, and that the seeming variety, in their effects, may, for the most part, be entirely referred to certain circum-

stances which give these causes more or less activity and force,—to the situations of the patients, and to the causes that concur in exciting them.

“ With respect, again, to fevers that arise from specific contagions, they are very few, so that the variety is by no means endless, as has been supposed. It has been imagined, indeed, that to characterise a specific contagion, it should affect persons but once in their lives ; but there are disorders, in other respects deserving to be considered as specific contagions, where this will not apply. Now, if this last character is given up, we shall find it probable that there is one general and common source of contagion producing continued fever, viz. the effluvia arising from the human body. This matter, however, may be in many different degrees of intensity or concentration, which will occasion some variety in the fever which results.

“ You will observe, however, that I give you only general propositions, and never one that is absolutely universal. So, on the subject of the Typhus, in a note, I say with respect to the long list of species enumerated, that, after all, I must allow there is a strong presumption that typhus may be of different species. This would appear from considering the Yellow fever of the West Indies, which seems to be of the nature of a specific contagion, it being always propagated with the same original appearance. The same may be said with respect to the sweating sickness of England, which I have thrown into the same place. But admitting these two to be different species of typhus, and, at the same time, considering that these are the whole that I can gather from different ages, it will lead us to conclude that the matter is of small variety.”

It is not improbable that Dr Cullen may have got his first impressions of the contagious nature of Yellow Fever from what he had heard of it during the six months he passed in the harbour of Portobello, in the

Gulf of Mexico (vol.i. p. 5), where the disease had raged and proved highly fatal three or four years before his arrival at that place. But it was not till some years after his death that the appearance of this disease in the West Indies, and in some parts of North America, began to excite that universal attention to its history, which has been almost uninterruptedly continued since its first appearance in Grenada, as described by Dr Chisholm, till its last appearance in Gibraltar, when it carried off, among so many others, that zealous and talented medical officer Dr Hennen, while employed in treating the disease and investigating its origin. During all this period, the contagious or uncontagious nature of yellow fever, has been the subject of much contention and controversy. As far as it is possible, amidst the conflicting and contradictory statements on matters of fact, which have been put forth in the progress of this controversy, to arrive at any concordant, consistent, or satisfactory conclusions, it would seem,

1st, That Yellow Fever, and that even of a severe and malignant character, is an endemial disease occurring in all warm climates sporadically, and in particular seasons even prevailing epidemically, in many instances without there appearing reason to believe that it has arisen from, or been propagated by, contagion.

2d, That the disease, in its severest forms, has most frequently appeared in Sea-port towns and their harbours, in which men were crowded together in narrow ill-ventilated streets and filthy houses; and that, originating in such situations, it has been supposed

by the non-contagionists to have been occasioned by local causes, whilst, in the minds of others, a belief has been produced of the disease having been imported, by particular ships, from places in which it was known or suspected to prevail at the time of these ships leaving them.

3d, That, when it has once manifested itself in these situations, whether it may have been of foreign or of indigenous origin, its diffusion has, by the one party, been attributed to the influence of the same local causes by which they believed it to have been developed, and, by the other party, to contagious communication.

These differences of opinion with respect to a point involving so many important consequences, have necessarily had the effect of calling into action all the talents, feelings, and interests of those who have engaged in its investigation ; and have unfortunately produced in many of them those states of mind in which the search after truth is so liable to be lost sight of in the desire for victory.

The question truly at issue between the contagionists and the noncontagionists, respecting Yellow Fever, has at length become, not, as has been often conceived and represented, whether this disease may not occur, and that of a malignant character, in an uncontagious form—for this seems to be now universally acknowledged ; but whether it has or has not been observed, in some particular instances, actually to spread by contagion. So many instances of such apparent communication have been recorded by credible persons, that the necessity of admitting the occasional contagious propagation of Yel-

low Fever can, it would seem, be evaded only by supposing a succession of very extraordinary mistakes or of strange coincidences. (Appendix, Note H.)

Entertaining the opinions already stated as to the limited number of the causes from which fevers arise, Dr Cullen was not disposed to accede to the doctrine which had been inculcated by Sydenham, that the fevers of the same district present an endless variety in the symptoms which they exhibit in different epidemics.

“After the observation of many years,” he remarks, “Dr Sydenham has concluded that the epidemics of every year differ essentially; so that, from his experience, he could not judge if the same kind of epidemic might, in the same rotation, appear again, and consequently could not pretend to ascertain the species of fevers and to fix the general method of cure. Now, there is no foundation for ascribing this variety in the characters of fevers to changes in any of the six non-naturals, though, perhaps from its being common to a number of persons, the variety may be referred to the atmosphere. But the inexplicable variety of the exhalations going on there, gives us hardly any hopes of attaining our purpose (*i. e.* of determining what is especially the variety of fevers); and the changes we see occurring in the sensible qualities of the atmosphere leave us without hopes of ascertaining this variety. So, if this doctrine be true, that the variety of exhalations in the atmosphere is inexplicable, and if diseases do so immensely differ, we cannot hope to be able to reduce epidemics to any common or general nature, and to find out a general plan of cure; but we must be obliged to remain attached to Dr Sydenham’s plan of studying every epidemic as it arises, and perhaps must destroy several lives before we can learn the method of treating the remainder. But his opinion on this subject is not well founded; for, from the observations I have delivered, the variety of these epidemics is evidently

not nearly so great as he alleged ; and as we may determine nearly to what length the variety proceeds, so we can reduce epidemics or epidemic fevers to certain genera, and thereby attain a generic method of cure."

The very limited variety of the causes which produce fevers, Dr Cullen accordingly professed to be a doctrine of which he was very fond ;—as it is, he remarks, " very favourable for putting the practice of physic upon a more steady foundation than that of Sydenham and Boerhaave. It will give some indications which will unite all the seeming diversity of remedies upon the general plan, and will correspond with the method of cure which experience has shewn to be most successful. Boerhaave has supposed that the *methodus sanandi requisita sæpe immensum differebat*, and if it was so, we could not hope to obtain this method of cure without the expense of many lives. But looking at the whole of Sydenham's works, we find no such variety ; only a few general remedies, employed more or less according to circumstances, but of no great variety ; so that the whole may be reduced to a few general rules, and these such as, in fact, he himself has given."

Previously to Dr Cullen's time, medical writers had very generally described, under the name of *Continent* or *Continuous*, a form of fever, the whole course of which, though protracted for several days, they supposed to consist of one single paroxysm, of but one hot fit, of only one reaction, uninterrupted by any remission. Dr Cullen, however, satisfied himself by observation, that every fever which is of more than one day's duration, is formed of repeated, and, in some measure, separate paroxysms, or exacerbations and remissions, though the remissions may be very obscure, and difficultly observed ; and, consequently, that there is no such fever as the schools had agreed to designate

Continent. "In the course of forty years' pretty extensive practice," says he, "I have never observed what I could call a continent fever. I could always discern, by attentive observation, more or less of exacerbation and remission." He satisfied himself, also, that each febrile paroxysm is finished in less than twenty-four hours, and that, consequently, every continued fever which runs out to more days than one, is formed of repeated paroxysms recurring in the course of twenty-four hours, or truly consists of diurnal exacerbations. This tendency in fevers to diurnal paroxysms, Dr Cullen thought, must depend upon the same law which seems to subject the human economy, in respect to many of its healthy or physiological phenomena, to a diurnal revolution, as shewn in the regular returns of sleep and waking, of appetites and excretions, and in the steady changes which may be remarked at different periods of the day, in the state of the pulse, and of most of the other functions of the body.

"But whether this diurnal revolution depends," Dr Cullen remarks, "upon the original conformation of the body, or upon certain powers constantly applied to it, and inducing a habit, I cannot positively determine. It may proceed from those causes which are produced by the diurnal revolution of the earth, viz. the regular alternation of heat and cold, light and darkness. But whatever its cause may be, I presume," he says, "from facts, that, in the human system, a diurnal revolution does take place, and that the phenomenon of the paroxysms of fever being always finished in less than twenty-four hours, does not depend on the cause of the fever, but rather upon the nature of the economy itself, or upon some law of the system determining it to a diurnal revolution, which modifies fevers in this respect. That febrile paroxysms

are connected with that diurnal revolution appears," he adds, "farther from this, that though the intervals of paroxysms are different in different cases, yet the times of the accession of paroxysms are generally fixed to one time of the day; so that quotidians usually come on in the morning, tertians at noon, and quartans in the afternoon." (Works, vol. i. p. 510.)

The occurrence of daily exacerbations and remissions in the course of continued fevers, seems to have been long partially known, though but little attended to by medical men. Dr Cullen has referred, in particular, to Vogel, De Haen, and Brendel, as bearing, in a greater or less degree, testimony to the truth of the fact; but it is certainly to Dr Cullen himself that physicians are principally indebted, if not for the original observation, at least for its first explicit statement and full accurate elucidation. The observations of more recent authors, as of Mongellaz, Bailly, Rayer, Puccinotti, and Pallas, seem sufficiently to establish that the tendency of diseases in which no intermission occurs, to periodical exacerbations, and that of diseases in which intermissions do occur, to periodical recurrences, are not confined to continued and intermittent fevers, but are observable in several other natural groups of diseases, nervous, inflammatory, hæmorrhagic, and dropsical.\*

Intimately connected with the question as to the tendency to diurnal revolutions or periodicity in the course

\* Mongellaz, sur les Irritations Intermittentes, &c.—Bailly, *Traité des Fièvres Intermittentes*, &c, 8vo. Paris, 1825.—Rayer, art. *Intermittent* in the *Diction. de Médecine*, xii. 336.—Puccinotti della Periodicità nelle Febbri e della sua Causa e Natura.—Pallas, sur l'Intermittence.



of fevers, is that relating to the doctrine of Crisis. This doctrine, as taught by Hippocrates in its application to various diseases, and particularly to fever, and adopted by a large proportion of physicians since his time, may be said to consist, in its simplest form, of three distinct propositions ; 1<sup>st</sup>, that the changes in the state of certain diseases, whether of a salutary or fatal character, occur, in a great proportion of cases, not in a gradual but in a sudden manner ; 2<sup>d</sup>, that these changes are very frequently attended by some very obvious external manifestation, as by great sweatings, diarrhoea, hæmorrhages, the formation of abscesses, &c. ; and 3<sup>d</sup>, that these critical phenomena, and consequently the changes in the state of the disease which they accompany or indicate, take place in a large proportion of instances on certain determinate days. These several propositions, in the Hippocratic doctrine, are not necessarily connected, so that the admission of any one of them does not necessarily require the admission of the others.

The opponents of the Hippocratic doctrine, as respects critical days, may be divided into those who deny that any such regularity in respect of the days on which salutary or injurious changes occur in diseases, had ever or in any place truly been observed ; and those who, in admitting the accuracy of the observations as made by Hippocrates in Greece, allege that, whether in consequence of differences in the character of the diseases, as influenced by climate, or in consequence of differences in the modes of treatment pursued, no such phenomena are observable at the present day, at least in temperate regions. The former of these opponents

derived a powerful argument from the contradictory statements in Hippocrates' own writings, as to what days are actually critical. This argument was accurately investigated by Dr De Haen of Vienna, who, in a chapter on that subject in the first part of his *Ratio Medendi*, published in 1756, gave, in a table, an analysis of the crises of the individual cases of acute diseases to be found recorded in Hippocrates' writings, according to the particular day on which a change, whether of a favourable or unfavourable kind, was observed to occur; incorporating in this table, as single observations, Hippocrates' statements respecting the days of termination of several epidemic constitutions. From this mode of testing Hippocrates' general statements on critical days, it appears that the 24th aphorism of the 2d Book of his *Aphorisms* corresponds best with his own observations as far as the 17th day, beyond which it does not extend; and that, in point of fact, the most critical days, according to these observations, are the 3d, 4th, 5th, 7th, 9th, 11th, 14th, 17th, 20th, and 40th. De Haen declares that in his own hospital practice at Vienna, exact observation had demonstrated the truth of Hippocrates' statements respecting these days.

Dr Cullen, in giving De Haen great credit for the judicious manner in which he had set about determining what were Hippocrates' real opinions, by appealing to the particular facts recorded in his writings, avowed himself an advocate for the existence and influence of critical days, as being satisfied that the doctrine of the ancients, and particularly that of Hippocrates, on this subject,—that is, of there being something in the

nature of fevers which generally determines them to be of a certain duration, or to terminate, whether in a salutary or fatal manner, at certain periods of the disease rather than at others,—was well founded, and that it is applicable to the fevers of our climate. With the exception of the 4th day, which he denied to be naturally critical, Dr Cullen adopted the enumeration of the critical days given by De Haen, as far at least as the 20th, beyond which he conceived that there are not a sufficient number of observations of fevers to ascertain their course, and that probably, also, the movements become less exact and regular, and therefore less easily observed.

As to the results of his own observations on the subject of critical days in fevers, Dr Cullen mentions in his First Lines “that they amount to this, that fevers with moderate symptoms, generally cases of the synocha, frequently terminate in nine days, and very constantly upon one or other of the critical days which fall within that period: But it is very rare in this climate that cases of either the typhus or synochus terminate before the eleventh day; and when they do terminate on or before this day, it is for the most part fatally. When they are protracted beyond this time, I have very constantly found that their terminations were upon the 14th, 17th, or 20th day.” (Par. 124.)

But in adopting the doctrine of critical days, as applying to the fevers of his own country and his own times, Dr Cullen did not admit as a necessary part of the doctrine of crisis, the occurrence of some unusual evacuation. “In such cases,” says he, “the salutary terminations are seldom attended with any considerable evacuations. A sweating frequently appears, but is seldom considerable; and I have hardly ever observed critical and de-

cisive terminations attended with vomiting, evacuations by stool, or remarkable changes in the urine. The solution of the disease is chiefly to be discerned from some return of sleep and appetite, the ceasing of delirium, and an abatement of the frequency of the pulse. By these symptoms we can often mark a crisis of the disease ; but it seldom happens suddenly and entirely : and it is most commonly from some favourable symptoms occurring upon one critical day, that we can announce a more entire solution upon the next following."

Those who have adopted the doctrine of critical days, have naturally been anxious to render some explanation of so remarkable a phenomenon. Hippocrates had adopted the Pythagorean opinions concerning the power of numbers, and to this circumstance may be attributed, if not the whole of his doctrine of critical days, as has been alleged by some, at least a part of the contradictions which appear, in different passages of his writings, in his mode of stating it. "I am to attempt," says Dr Cullen, in his Lectures on the Practice of Physic, " a solution of this seemingly difficult problem, and that from the laws of the animal economy ; but though the account I am to offer may be frivolous, and even fallacious, this will not, in the slightest degree, weaken the force of the facts I have alleged."

The principles of Dr Cullen's explanation of this tendency of continued fevers to terminate at certain periods of their course, were, 1<sup>st</sup>, that this tendency is connected with a general disposition of the animal economy to periodical movements ; there being few continued fevers in which the most heedless observer may not perceive periodical paroxysms : and, 2<sup>d</sup>, that

from the universality of *tertian* or *quartan* periods (§ 119) in intermittent fevers, we cannot doubt of there being in the animal economy a tendency to observe periods of these types. “Of a thousand cases of intermittent fevers,” said he, “the *tertian* type certainly occurs in upwards of nine hundred; and, from that type most frequently occurring, there can be no doubt that there is a law of the system thus determining it, though in this, as in many other instances of the laws of the animal economy, which we can only take up as so many facts, there is, or may be, in every one of them some anomaly or irregularity. They are all established by the majority of instances, when that is considerable, as, perhaps, nineteen cases out of twenty; and however we may account for the irregularities, certainly the general fact is determined by a law of the system. The tendency of the animal economy is to a *tertian* period, and the only remarkable exception is when the period proves *quartan*, which certainly occurs, filling up a tenth, or perhaps only the twentieth part of the cases; but it is still so frequent as to be admitted as a natural period. The critical days above mentioned are consistent with this tendency of the economy, as all of them mark either *tertian* or *quartan* periods. These periods, however, are not promiscuously mixed, but occupy constantly their several portions in the progress of the disease; so that, from the beginning of the disease, a *tertian* period takes place; and from the eleventh to the twentieth, and perhaps longer, a *quartan* period is as steadily observed. What determines the periods to be changed about the eleventh day, we have not clearly perceived.”

From a statement by Dr George Fordyce in his third Dissertation on Fever, it appears that so early as 1757, the year after the publication of De Haen's Essay, Dr Cullen had suggested the explanation of critical days, stated in the above extract. “The only plausible mode of accounting for these critical days,” says Dr Fordyce,

“is that which was pointed out by Dr Cullen, in a clinical lecture he gave about the year 1757 ; viz. that continued fevers observe, in some degree, the types of intermittents. He said that, during the first week of a continued fever, it observed the quotidian type ; that, in the second, it observed the tertian type ; and in the third week, the quartan type,” &c. The author of a criticism of the First Lines in the Journal de Médecine says, in reference to the account of critical days given by Dr Cullen, that “ his idea on this point of doctrine appears the most simple and the most luminous that has ever been proposed, all difficulties disappearing before it, and doubt being changed almost into certainty.” And Dr Mason Good remarks, that “ Dr Cullen examined this subject with great attention, and simplified it from many of its difficulties ;” and that “ Dr Fordyce, who scarcely does justice to Cullen upon other points, unites with him upon the present, and justly compliments him upon his ingenious examination and explanation of the Greek distribution of critical days.”

Another curious speculation relative to the influence of periodical revolutions on disease, is the doctrine which has been entertained by a number of physicians and philosophers, from Hippocrates and Aristotle downwards, that many diseases, and particularly fevers, are influenced in respect of their commencement and of their course, by certain relative positions of the sun, moon, and earth. Dr Francis Balfour (who graduated at Edinburgh in 1767), having published in Calcutta, in 1784, a small treatise in which he endeavoured to call the attention of the medical profession

to several propositions respecting the influence of the moon in fevers, this treatise was, by Dr Cullen's advice, reprinted at Edinburgh in the following year. (See Mr A. Balfour's letter to Dr Cullen, vol. i. p. 643.) On his return to England, the author, as is well known, submitted to the public several additional works in corroboration of the doctrine of sol-lunar influence.\* The republication of Dr Balfour's first essay, by the recommendation of Dr Cullen, induced the late distinguished army physician Dr Robert Jackson, to address to Dr Cullen, a letter containing a statement of his own observations on the connection subsisting between the moon's periods and the invasion and relapse of fevers. Though Dr Jackson soon afterwards published his observations on this subject at greater length,† and resumed its consideration in several of his subsequent writings, his letter, as containing a succinct view of the facts on which his opinions were originally founded, has appeared to me deserving of a place in the Appendix. (Note I.)

Having, under the head of Prognosis, established three diathetical states of the system as liable to occur in fevers, each of which is capable of producing the ten-

\* *Treatise on Putrid Intestinal Remitting Fevers*, Edinburgh, 1790.—A collection of *Treatises on the Effects of Sol-Lunar Influence on Fevers*. Lond. 1812. Several East India practitioners who have not adopted Dr Balfour's opinions, have, however, admitted the general accuracy of his facts, and have endeavoured to explain them by the influence of the moon on the tides, and the effects of the water of these upon the soil and subsoil on the borders of the sea-shore or of rivers.

† *Lond. Med. Journ.* for 1787, vol. viii. part 1. (See also Dr Balfour's collection of *Treatises*, p. 278 and 354.)

dency to death,—those viz. of violent reaction, great debility, and a tendency to putrefaction in the fluids, Dr Cullen conceived that the indications of cure should be formed upon the view of obviating the tendency to death, from whichever of these states it may seem to proceed, the means of executing these indications being directed by a proper attention to the proximate cause of fevers. Accordingly, he considered that there are three general indications to be followed in the cure of continued fevers, viz. to moderate the violence of reaction, to remove the causes or obviate the effects of debility, and to obviate or correct the tendency of the fluids to putrefaction. It appears from notes of the course of lectures on the Practice of Physic which he delivered during the session 1771-72, that the table inserted at the 227th section of his First Lines, in which the whole of the cure of continued fevers, as founded on these three indications, is brought under one view, was circulated amongst his students in the course of that session ; and it likewise appears that, in commenting on that table, he took a very full view both of the Stimulant and of the Debilitating plan of treatment in fevers, under the two titles of—“ To take off the excess of stimulant power,” and, “ To obviate debility or take off the excess of sedative power.”

With respect to the third indication of cure—that of obviating or correcting the tendency of the fluids to putrefaction—it is obvious that the importance attached to it must depend, in the first place, upon the opinions that may be entertained as to the actual occurrence of any change in the fluids of the body in the progress of fever, and particularly of any change that can be re-



garded as denoting a tendency to putrefaction in the fluids ; and, secondly, supposing such tendency to occur, on the opinion that may be entertained as to the possibility of obviating it. Dr Cullen, though he has been represented by several of his critics as an “ exclusive solidist,” had expressly stated, in his First Lines (§ 50), that whilst he conceived there is no evidence in support of the old doctrine of a viscosity subsisting in the mass of blood previously to the cold stage of fever, by which the production of this state can be explained, he by no means intended to deny that the cause of fever frequently operates upon the fluids, and particularly produces a putrescent state of them. “ I acknowledge,” says he, “ that this is frequently the case.” The enumeration given by him, in the 105th paragraph of the First Lines, of the symptoms denoting the putrescent state of the fluids, and, in the 222d to the 226th paragraph, of the means of obviating and correcting their tendency to putrefaction, is well worthy the consideration of those who entertain any doubts as to the fact of the animal fluids undergoing considerable modifications in their physical properties and chemical constitution in the progress of fevers. But, in truth, the more recent researches of Clanny, Stoker, Burne, Tweedie, &c. relative to the conditions of the blood in typhus fever, as occurring in these islands ; those of Chomel, Andral, Louis, Lecanu, &c. relative to these conditions in the continued fever of France ; and those of Dr Stevens regarding them in the ardent and malignant fevers of hot climates, confirmatory as they are of the observations of Towne, Warren, Hume, Hillary, &c. leave no room for doubt as to the actual occurrence

of such changes, whatever opinion may be entertained as to the precise period in the course of fevers at which they occur, or as to the precise chemical alterations upon which they depend. At the same time, Dr Cullen certainly regarded the other two indications of cure which he established—the first directing the employment of the means that are calculated to moderate the violence of reaction, and the second the employment of those that are fitted to remove the causes or obviate the effects of debility—as more important, and as requiring, in their fulfilment, the greatest skill on the part of the practitioner. It may be doubted whether any author, before or since his time, has pointed out so distinctly as he has done, the different circumstances in fevers which require the employment, respectively, of the debilitating and of the stimulating modes of treatment, particularly in regard to the abstraction of blood, and to the use of wine and of opium. His distinct and explicit statements on these points, however, have not had the effect of preventing many erroneous representations from being given of his opinions relative to the treatment of fevers.

“ We see, then,” says Dr Welsh, in his *Practical Treatise on the Efficacy of Bloodletting in the Epidemic Fever of Edinburgh* (1819), “ that primary debility in fever was a necessary part of Dr Cullen’s doctrine ; and the debility might be so great as entirely to exhaust the powers of life, that is, to produce death, or, though it did not proceed quite so far, might readily be carried to that extremity by the exhaustion produced by any additional sedative applied ; and hence it became a dangerous matter to prescribe sedative means that might co-operate too far or too effectively with the remote cause of the disease. Another, and the

chief, fallacy was in changing the name of febrile excitement into *reaction*,—a change which, by presupposing a debilitating power to which it was opposed, necessarily implied a danger of uncertain extent to be dreaded, and which must, of course, be augmented by the only means capable of removing the bad effects of reaction, that is, by the more powerful evacuates. In consequence, we find this great Professor manifestly averse to bloodletting, nearly interdicting purgatives (146, 147, 148), and exceedingly shy even of his favourite remedy, emetics (178, 179), in continued fever."

Dr Welsh also says (p. 117), that "Cullen, perhaps unknown to himself, succeeded in nearly interdicting the use of bloodletting in fever; for it must be confessed, that his precepts on that subject are so hedged in with *provisos*, that one can hardly suppose he seriously meant it to be employed." Dr Welsh's opinions on bloodletting in fever were formed from the observation of a single epidemic,—Dr Cullen's from the observation of many, and the careful comparison of his own personal observation with the recorded experience of others. No wonder that their conclusions were different; and no wonder if it be true that, short as Dr Welsh's life was, it was long enough to afford him the opportunity of becoming satisfied how very unsafe it would be to treat contagious fever, whenever it presents itself, on the same plan which was found so beneficial in the epidemic which he has so well described, and the leading character of which was known to be a strong, or even violent, reaction early in its occurrence and progress.

In the article Fever in the Cyclopædia of Practical Medicine, in speaking of the employment of those means which induce perspiration for the purpose of

procuring an abatement of the febrile heat, it is observed : “ This attempt to imitate nature was strongly inculcated by Cullen. Spasm of the extreme vessels being the supposed cause of the phenomena of fever, it was imagined that every measure which tended to diminish or remove this state of the cutaneous exhalants, would have the effect of subduing the disease. Upon these grounds, the employment of Diaphoretics in fever was suggested, and these remedies now constitute a part of the treatment of all febrile diseases.” A perusal of the 162d to the 169th paragraphs of Dr Cullen’s First Lines, will shew how very erroneous the above representation of his views respecting the employment of sudorific medicines in fever is ; with how much impartiality he has stated the arguments in favour of and against the practice of sweating in fevers ; with how much discrimination he has distinguished between the means of fulfilling this indication that are safe, from those that are attended with mischief or danger ; and has laid down the rules for the conduct of sweating, if the practice is to be attempted ; and with how much modesty he has declared, that it must be left to further experience to determine how far any general rule can be established in this matter.

Dr Currie states in his Medical Reports (vol. 1. p. 90, 5th edition), that “ Dr Cullen mentions cold drink, but gives no opinion on the propriety of its being used in fevers, and certainly did not recommend it in practice. He was even doubtful of the extent to which cold air might be admitted ;” and, in corroboration of this statement, Dr Currie in a note refers to the First Lines, article Cure of Fever.

In his First Lines, Dr Cullen, in reference to the employment of cold as a tonic in fevers, remarks, that “the experience of all ages has confirmed that cold drink taken into the stomach has proved very hurtful, and, therefore, that the use of cold drink in fevers requires some limitations. What these limitations should be, and what are all the circumstances which may forbid the use of cold drink, is difficult to determine; but it seems clearly forbidden in all cases where a phlogistic diathesis prevails in the system, and more especially when there are topical determinations of an inflammatory nature.” In his lectures he entered upon this subject more fully. After referring to different ancient and modern authors, he remarks, —“From these authorities it would appear that, whenever the character of the fever is ambiguous, or whenever it is a synochus with some degree of inflammatory diathesis joined, and we are uncertain how long that may subsist, especially where that suspicion arises in cold climates or cold seasons, and in general during the spring season, when there is the greatest prevalence of inflammatory diseases, the use of cold drink is a suspicious practice. I would even admit, further, that, from the like reasons and suspicions, it is of doubtful use in the beginning of most fevers, and is at least to be employed with a great deal of caution. This consideration will sufficiently explain to you why the practice of this country is, so universally, that of giving warm drinks in fevers. But, to be sure, our cautions have been carried too far, and have missed the benefits which we might have obtained; and, for reasons of a contrary kind, it is evident that, in all fevers that are purely nervous, or are manifestly accompanied with a putrid diathesis, cold drink must be useful and even necessary; and, accordingly, it will equally follow that, in hot climates and seasons, and particularly in the autumnal fevers of all climates, cold drink is safely applicable, and is one of the most useful remedies. Nay, I wish we had the

courage, and stood so little in awe of popular prejudice, as to admit it in the advanced state of most fevers when the inflammatory diathesis which did prevail is either entirely gone or considerably abated, or whenever we can have any tolerable security that the system is without inflammatory topical determinations to the lungs or to the brain. Upon the whole, there is no sort of doubt, that the use of cold water may be one of the most powerful remedies in fevers, and we have, both by design and accident, innumerable instances of its efficacy."

On the subject of exposure to cold air, of which Dr Currie states that Dr Cullen was doubtful of the extent to which it might be admitted in fevers, Dr Cullen observes, in his First Lines, that "when the power of the system generating heat is increased, as is commonly the case in fevers, it is necessary not only to avoid all means of increasing it further, but it seems proper also to supply air of a cooler temperature, or at least to apply it more entirely and freely, than in a state of health. Some late experiments in the smallpox and in continued fevers, shew that the free admission of cool air to the body is a powerful remedy in moderating the power of reaction; but what is the mode of its operation, to what circumstances of fever it is peculiarly adapted, or what limitations it requires, I shall not venture to determine, till more particularly instructed by farther experience."

In his First Lines, Dr Cullen adverted to the fact that not only cool air, but cold water also, may in fevers be applied to the surface of the body, as a refrigerant and perhaps as a tonic remedy. "The ancients," he observes, "frequently applied it with advantage to particular parts as a tonic; but it is a discovery of modern times, that in the case of putrid fevers, attended with much debility, the body may be washed

all over with cold water.” In his lectures, Dr Cullen was accustomed to enter into an historical detail of the employment of cold washing or cold bathing in cases of fever, referring particularly to the treatise of Dr De Hahn of Breslaw, the original of which, however, he does not appear to have seen. From that detail he conceived it plainly to appear, that from both the ancients and the moderns we have enough of facts to entitle us to consider cold applied to the surface of the body as a remedy of some importance, and that it may be employed wherever the employment of cold water as a drink is admissible. It is unnecessary to say how strong a confirmation this statement subsequently received in the Reports of Dr Currie, to whom, as he has himself very candidly stated, a trial of the practice was first suggested by the perusal of a little memoir on the subject by the late Dr William Wright of Jamaica, and latterly of Edinburgh.\*

Those who have been led to believe that Dr Cullen was a mere theoretical physician, unacquainted with diseases as they present themselves in nature, and speculating upon them in his closet from the information supplied by the writings of others, and that his own writings exhibit rather the workings of his imagination than the results of his personal experience, would do well to peruse the series of cases of fever

\* That judicious practitioner and excellent naturalist having satisfied himself of the beneficial effects of the external use of cold water in the smallpox, during the epidemic prevalence of that disease in Jamaica in 1768, was led to make trial of it upon himself on the occasion of his being seized with contagious fever when on his passage from Jamaica to England in 1777.

which he was accustomed to sketch in his lectures (Works, vol. i. p. 652, *et seqq.*), in order, as he says, to illustrate his doctrine with respect to the treatment of these diseases, and to give the particular application of it. No person, it is conceived, who has himself had experience in the treatment of fevers, can rise from the perusal of these few pages with a doubt upon his mind as to their having been written by a man who, to a most logical mind, united singular powers for the accurate observation of the phenomena of diseases, and of the changes induced in them by different modes of treatment, and who, by the diligent and long continued exercise of these powers, had rendered himself a consummate medical practitioner. It is much to be regretted that Dr Cullen should not have embodied his views of fever in a separate and general treatise on that subject. His lectures contained ample materials for such a work ; and no one could have arranged them in so perspicuous and accurate a manner as he would himself have done.

Some years before the appearance of the first volume of the First Lines (*viz.* in 1772), Dr Lettsom, who had been a pupil of Dr Cullen's a short time before, published anonymously a small treatise entitled *Reflections on the General Treatment and Cure of Fevers*. The opinions expressed in this treatise respecting the diseases of the Fluids, respecting Marsh Miasma and Human Contagion, respecting the tendency of these morbid agents to induce debility of the system by suddenly weakening the nervous power and energy of the brain, together with several other opinions both theoretical and practical, shew how largely Dr Lettsom, in



compiling this treatise, had drawn upon the lectures of Dr Cullen. That in freely availing himself of these he made no reference to the source from which he derived them, is calculated to create a very unfavourable opinion of Dr Lettsom's candour.\* It is satisfactory, therefore, to be able to state his apology in his own words.

Dr LETTSOM to Dr CULLEN.

LONDON, GREAT EASTCHEAP,

. HONOURED PROFESSOR,

August 22. 1773.

" I know thou art not a stranger to a publication I lately made, entitled *Reflections on Fevers*, the first and only edition of which is now nearly sold. I write this, therefore, to request the favour of Dr Cullen to permit me to dedicate to him the next edition. It is true, the former was inscribed to Lord Dartmouth, but as I am now about publishing a work to be more immediately under that nobleman's sanction, I can with propriety omit his name in the treatise on fevers.

" I am conscious how much information I have received from Dr Cullen, but I thought it would be unjustifiable to ascribe to him doctrines which were neither generally admitted nor generally known.

" It appeared to me, therefore, more prudent to risk my character, rather than to attribute those things to another, which I had no authority to publish as such. But now

\* This treatise is noticed in the first volume of the *Medical and Philosophical Commentaries*, by a Society in Edinburgh, published in the beginning of 1773, in the following terms:—" No name is printed on the title-page of this treatise; nor could the author with justice assume to himself the merits which may be due to those new opinions it contains. We are here presented with a concise, but tolerably distinct view of the doctrine of fevers which has, for several years past, been delivered by one of the professors of medicine in this University."

the circumstances widely differ, as both the Monthly and Critical Reviews, as well as the Public, have expressed such approbation of the first outlines, as induces me to presume to ask Dr Cullen's sanction of the improved edition, which I am now preparing for the press.

"Though this little work hath had a hasty sale, I should not have attempted a second edition so early, were it not from a desire to acknowledge openly the obligations I owe to Dr Cullen, and likewise to gratify Dr Macquer, and Dr Dubourg of Paris, who are translating my piece into French, and wait for the second edition to conclude the same, as the former has just informed me. I beg Dr Cullen would indulge me in this favour, to be informed of which, as soon as convenient, will greatly oblige his sincere admirer,

"JOHN COAKLEY LETTSOM."

Dr Cullen's reply to this communication is peculiarly interesting as a statement of his wishes regarding the publication of his opinions by his students.

Dr CULLEN to Dr LETTSOM.

EDINBURGH, *3d September 1773.*

MUCH ESTEEMED FRIEND,

I am sorry that both absence from town and much business have prevented me from answering your polite letter so soon as I wished.

"I must own to you that I thought myself entitled to some acknowledgments in your former publication, but I willingly accept of your excuse. I am not very vain of my own opinions, and am indeed not willing that my pupils should point them out very particularly, as I am not confident enough in giving them to the public, especially in the imperfect state in which I am sometimes obliged to deliver them, or in the imperfect manner in which they are sometimes taken by my pupils. However, I must also say that I am always proud of having given any light or instruction

to persons who can make so good use of them as you can, and make so candid acknowledgments as you do. Upon this footing, I must say that I shall think myself much honoured by your dedication. I am very glad to find, by your last publication, that some opinions which I was afraid of hazard-ing have been so well received by the public. I hope your new work shall have equal success, and you may depend upon it that, when it shall appear, I will do all justice to your merit in it, which, from the former essay, I believe, will be very great. Believe me to be, with regard and esteem, dear Sir, your faithful and most obedient servant,

W. C.

“ P. S.—I need hardly tell you that I am now engaged entirely in teaching the Practice of Physic, and my whole studies will be directed to it. I hope to go farther than I have yet done in the doctrine of fevers, and hope also to communicate it to the public ; but, amidst my many occupations, and under increasing years, it is very uncertain when, or if ever.”

The other Pyrexial diseases besides Fevers, those, namely, which are necessarily connected with, and dependent upon, local affections, Dr Cullen referred to four orders, according as the local affection consists in inflammation, cutaneous eruption, hæmorrhage, or excessive discharge or excretion.

Inflammatory diseases, or the Phlegmasiæ, as they were designated by Dr Cullen, in following Sauvages, constitute a natural family which, from their number, and the importance of their effects upon the economy generally, and on particular parts of it, as well as from their frequently requiring prompt and vigorous measures of treatment, must always be regarded as of the highest interest to the medical practitioner.

Dr Armstrong, following Dr John Brown, complains

that Dr Cullen, “ in his order Phlegmasiæ, has made the fever appear always as the mere effect of inflammation ; but if you investigate this hypothesis, you will find it to be erroneous, for, even in this order, inflammation is more frequently the consequence than the cause of the fever, as I shall afterwards prove.” In discussing the doctrines of Brown, I shall have occasion to refer to the view which Dr Cullen took of the relation in respect of priority of appearance, of the local and constitutional affections, not only in the Phlegmasiæ, but in some of the other orders of the Pyrexia. At present, I shall only observe that the occasional or frequent supervention of inflammation in the progress of a fever, certainly does not disprove the liability of fever to supervene in the progress of a primary or protopathic inflammatory affection.

In respect of the Proximate Cause of Inflammation, Dr Cullen set aside the idea entertained by Boerhaave, of its depending on an obstruction produced by a preternatural lentor or viscosity of the blood, or by particles of blood entering a vessel or vessels whose branches will not allow of its passage. And, in conformity with his general views as to the dependence of the phenomena of the animal economy, healthy and morbid, on the living solids, he conceived that a spasm of the extreme arteries, supporting an increased action in the course of them, may be considered as the proximate cause of this state.

His doctrine on this subject may be regarded as consisting of the following propositions :

1st, That the phenomena of Inflammation depend

immediately on an increased impetus of blood in the vessels of the part affected.

2*d*, That this increased impetus is owing especially to the increased action of the vessels of that part itself.

3*d*, That the increased vascular action manifestly arises in many cases from the application of stimulant substances to the part, but, in other instances, no such application being evident, some other cause must be sought for.

4*th*, That though obstruction from a matter stopping up the vessels is not to be considered as the primary cause of inflammation, it is probable that some degree of obstruction does take place in every case of inflammation, which obstruction produces an inequality in the distribution of the blood.

5*th*, That an unusual quantity of blood being thrown upon particular vessels, necessarily proves a stimulus to them ; and

6*th*, That the *vis medicatrix naturæ*, by the formation of a spasm on the extremities of these vessels, still more increases their action, the tendency of which is to relieve the congestion.

Diversified as have been the explanations given by different pathologists, since Dr Cullen's time, of the nature of the successive changes in the vascular system on which the production of inflammation depends, they have very generally concurred with the doctrine of De Gorter, followed by Dr Cullen, in this respect, that they have sought in the containing solids rather than in the contained fluids, for the sources of the

obstruction which the external phenomena prove the blood to undergo in the part that is, or that becomes, the seat of inflammation. With the exception, indeed, of the first of these propositions, representing the increased impetus of the blood as occurring in the vessels of the part inflamed, Dr Cullen's doctrine of Inflammation corresponds with the results of the latest and best directed inquiries into this subject.

With respect to the recognition of the influence of the *vis medicatrix* in the production of inflammatory spasm, without which it seems difficult to account for the increased pulsation in the part affected, precisely the same observations in the way of objection and of explanation, are applicable here as were made in reference to the same theory of Febrile Spasm.

The investigation of the local phenomena and effects of inflammation in the successive stages of its progress, carried on so indefatigably and successfully for a long series of years by Mr Hunter,—but which were not published till after his death,—and afterwards so happily extended by the genius and industry of Bichat, to the modifications to which inflammation is subjected in the different textures, systems, and organs of the body—may be said to have laid the true foundation for a great part of those discoveries in pathological anatomy which chiefly characterize the medical inquiries of the present period. That Dr Cullen, however, was not unaware of the effects of texture in modifying the phenomena of inflammation, is evident from the arrangement which he made of inflammatory diseases into Cutaneous, Vis-

ceral, and Articular, and from the accuracy with which he points out the three distinct seats which inflammation may occupy, when it attacks the viscera contained within the cavities of the chest and abdomen, viz. their serous and mucous membranes, and their parenchymatous substance.

The influence of the natural texture or structure of the part inflamed, as one of the principal causes of specific distinctions amongst the various forms of inflammation, had been particularly pointed out to the notice of the profession in this country some years before the appearance of Bichat's *Traité des Membranes*, by a distinguished pupil and friend of Dr Cullen's, Dr, afterwards Sir, James Carmichael Smyth, in a paper read to a Society for the promotion of Medical Knowledge, in 1788, and published in 1790.\* In this paper, which is entitled, "Of the different kinds or species of Inflammation, and of the causes to which those differences may be ascribed," Dr Smyth particularly considered the inflammations of the skin, the cellular membrane, the diaphanous membranes, the mucous membranes, and muscular fibres. It is to the volume containing this memoir, that Dr Carmichael Smyth refers in one of the letters addressed by him to Dr Cullen that are inserted in the Appendix to the former volume of this work (p. 630). To the several touching expressions of attachment contained in these letters,—equally honourable to him from whom they proceeded, as to him to whom they were addressed,—I have much

\* Lond. Med. Communications, vol. ii.

pleasure in adding the following, contained in a note in the second volume of the London Medical Communications.

“ I quote Dr Cullen as one of the latest and most respectable authors, the mention of whose name always gives me a singular pleasure, as it brings to my remembrance the many advantages I derived from his instruction, and the happiness I long enjoyed in his society and friendship. *Ἡγησάμην μὲν τοὺς διδάξαντά με τὴν τέχνην ταύτην ἰσὰ γενέσθαι ἐμοῖσιν.* Hippocr. Jusjurand.”

Great and valuable as the additions made to our knowledge of inflammatory diseases, since the publication of Dr Cullen's First Lines, have unquestionably been, a comparison of the accounts which he has given of this class of diseases with those of later writers, will evince that these additions have hitherto tended more to the improvement of the diagnosis and histories of particular inflammatory diseases, than to the introduction of any important changes in the modes of treating them. It is curious to find that even the mode of treating inflammatory affections, particularly of the Chest, by the internal use of large doses of emetic tartar, which has of late years been so strongly recommended by some Continental physicians, had been made the subject of clinical experiment and remark by Dr Cullen in the Royal Infirmary of Edinburgh. In a clinical lecture delivered by him 5th April 1768, formerly referred to (see vol. i. p. 114), in describing a case of fever with inflammatory diathesis, in which the patient had been bled three times with but little relief, and in which Dr Cullen was averse to the repetition of bloodletting, he observes,—“ We trusted to emetic



tartar and blistering. Neither four grains given in twelve hours on the 10th, nor two grains given in three hours on the 11th, had any effect on the disease, because the intervals were too large or the doses too small, but more especially the first. But doses of one grain each, given every half hour to three times on the 12th, and to five times on the 13th and 14th, brought down the pulse, and made it fuller, firmer, and more equable, induced sleep, and took off delirium."

Among the inflammatory diseases, Dr Cullen included the Gout, and from the bearings of this disease on his general system, he was led to bestow on it much consideration; and he certainly seems to have discussed it in a more simple, and at the same time more precise, manner than had been done by any preceding writer. The opinion which Dr Cullen was led to form respecting the pathology of this disease differed so widely from those entertained at the time he began to teach the Practice of Physic, as to procure for it no inconsiderable degree of attention.

"The opinion," he remarked in his lectures (See Works, vol. ii. p. 120), "which has generally prevailed is, that the gout depends upon a certain morbid matter always present in the body, and that this matter, by certain causes thrown upon the joints or other parts, produces the several phenomena of the disease. Stahl is the only writer who has thought that the supposition of a morbid matter was not necessary, and that the disease could be better explained by the state of the motions of the system. I very early adopted this opinion of Stahl: the contrary, however, is generally received; and when persons wished to shew the absurdity of my opinions, they thought that this was the strongest proof

of it, that I denied the existence of a morbid matter in the gout."\*

Sir Charles Scudamore, in a professed treatise upon Gout, has made a number of criticisms on Dr Cullen's definition and history of this disease and its different forms, which it is proper to notice here ; their tendency being to induce the belief that Dr Cullen had not duly considered the subject. Sir Charles objects, in the first place, to Dr Cullen having introduced into his definition of gout the character of a "hereditary disease," alleging that the gout is found to be more frequently acquired where no hereditary reference can be traced, than where such influence does exist. In his First Lines, Dr Cullen had himself restricted his definition by observing (§ 493) that "the gout is generally a hereditary disease, but some persons, without hereditary disposition, seem to acquire it ; and in some a hereditary disposition may be counteracted by various causes. These circumstances," he adds, "may seem to give exceptions to our general position, but the facts directly supporting it are very numerous." The proportional number of cases in which gout appears as a hereditary or as an acquired disease, will probably in a great measure depend on the state of society at the particular time or in the particular country. Where society is very stationary, wealth remaining confined to the same

\* As a proof of the difficulty with which the opinion of the non-existence of a morbid matter in gout was received by medical men, it deserves to be mentioned that Dr Cullen's theory of gout was opposed on this ground in an inaugural thesis, published by Dr Tode at Copenhagen in 1784, and in another, published two years afterwards at Halle, by Dr Luther.

families from generation to generation, the hereditary character of gout will be most marked. Where, again, by manufacturing skill or commercial enterprise, many persons are raising themselves from the necessity of active labour to affluence and its accompanying ease and luxuries, a large proportion of cases of acquired gout may be expected to occur.

A second character in Dr Cullen's definition of Gout to which Sir C. Scudamore objects, is that of its "arising without any evident external cause," the gout being, according to him, often called into action, even in the first fit, by a cause equally external and evident as that which leads to rheumatism. In his First Lines, Dr Cullen has observed that "physicians have been very confident in assigning the occasional causes of gout; but in a disease depending so much upon a predisposition, the assigning occasional causes must be uncertain, as in the predisposed the occasional causes may not always appear, and in persons not predisposed they may appear without effect." In pointing out what he thinks may be considered as the occasional causes of this disease,—which are, for the most part, of an internal description,—he mentions cold applied to the lower extremities; and in his lectures, in pointing out the grounds of distinction between gout and rheumatism, he observes,—"One cause they have in common, viz. sprains; for, if desired to recollect, most patients attribute to this the first fit of the gout, as well as of the rheumatism."

It is not meant by these observations to deny that Dr Cullen's definition of gout, as given in his Nosology, might be improved by being expressed in a more

restricted manner, in respect of the two characters objected to by Sir C. Scudamore ; but merely to shew that the exceptions were not overlooked by him in his writings or in his lectures.

Sir C. Scudamore objects to Dr Cullen's division of gout into Regular and Irregular, on the grounds that "the fundamental distinctions convey, on the one hand, more precision of definition than the various modifications which the disease assumes, seem to admit ; and, on the other, allow more looseness and latitude of application than is compatible with good practice." "An attack of gout," he adds, "is not the less regular because it seizes some other part not belonging to a joint, or appears in some other situation instead of the foot and hand, or because it proves of long duration, and changes frequently its seat." Sydenham had given the name of Irregular to gout when it appeared in any other situation than the foot ; but this was not the signification in which that term was adopted and understood by Dr Cullen, who used it rather as equivalent to the term *anomalous*, as employed by Musgrave ; that is, according to Musgrave's own representation, as synonymous with Internal. In the 518th paragraph of his First Lines, Dr Cullen has stated, in very clear language, the grounds of his distinction between regular and irregular gout, leaving, it is conceived, no room for the very vague objections urged against it by Sir C. Scudamore.

"In the whole of the history already given," says Dr Cullen, "I have described the most common form of the disease, and which, therefore, however diversified in the manner I have said, may be still called the *regular* state of the gout. Upon occasion, however, the disease assumes different ap-

pearances ; but as I suppose it always to depend upon a certain diathesis or disposition of the system, so every appearance which we can perceive to depend upon that same disposition, I still consider as a symptom and case of the gout. The principal circumstance in what we term the regular gout is the inflammatory affection of the joints ; and whatever symptoms we can perceive to be connected with, or to depend upon, the disposition which produces that inflammatory affection, but without its taking place, or being present at the same time, we name the *irregular gout*."

Sir C. Scudamore objects to the introduction into the definition of regular gout, of the characters, that the inflammation of the joints continues for several days, and recedes gradually with swelling, itching, and desquamation of the part. "The time of duration here mentioned," says he, "belongs almost only to the first fit ; and in regard even to this, much exception prevails. The swelling is not deferred till the inflammation has gradually receded, but usually takes place in the course of twenty-four hours, and is rarely delayed beyond forty-eight. The itching, as being the last and least remaining irritation of nerve, and also incidental to a scurfy state of the skin, is an ordinary occurrence ; but the desquamation of the part is by no means sufficiently frequent to serve as a characteristic symptom."

It is quite obvious that the characters here mentioned by Dr Cullen are to be regarded as an enumeration of the circumstances by which the external inflammatory affection, occurring in a *regular* attack of gout, may be distinguished from the inflammatory affection of the same parts in an *irregular* attack ; that they are intended to mark the contrast between, *first*, the degree of the inflammation ; *second*, its duration ; and, *third*, the state in which it leaves the integuments

upon its disappearance. Amidst the great diversity which unquestionably occurs in particular instances, there seems no reason to doubt that these characters correctly mark this distinction. The inflammation in the regular gout recedes gradually—in the irregular, suddenly; in the regular, it leaves swelling, &c. behind it—in the irregular, no trace. Sydenham distinctly mentions, that, “when the fit is going off, a violent itching seizes the affected foot between the toes, and afterwards the skin of it peels off by scales.”

Dr Cullen's definition of Atonic, as one of the forms of irregular gout, is also censured by Sir C. Scudamore. That definition recognises two classes of cases, *1st*, those in which the symptoms consist solely of debility of the stomach, or other internal part, without the expected or usual inflammation of the joints; and, *2d*, those in which the symptoms consist of pains in the joints that are only slight and transient, and that often alternate quickly with dyspepsia or other symptoms of debility. The latter class of cases Sir C. Scudamore recognises as constituting what he names the Passive or Chronic form of gout. But of the part of the definition which applies to the former class of cases, he says, that “it is descriptive only of the dyspepsia, or other internal derangements, which may take place in the gouty individual as a common occurrence, and without being necessarily dependent on that state of the system which tends to gout.” But the practical question obviously is, whether, when the stomachic affections alluded to, take place in a gouty individual, they be not in a great majority of instances, if not invariably, dependent upon the ar-

thritic diathesis, or modified by it, even although no local inflammatory affection occurs. That they really are so is apparent not only from the great liability of such symptoms to occur in persons of a gouty diathesis, but also from circumstances manifesting themselves in their progress, as well as from the mode of treatment which they require. Unquestionably, the suspicion of their gouty nature will be greatly strengthened, as Dr Cullen has noticed in § 521, when the symptoms mentioned are intermixed with, and are relieved by, some degree of the inflammatory gout.

But Sir C. Scudamore's most important objections to the view of gout taken by Dr Cullen, seem to have had their origin in his inattention to the distinctions which Dr Cullen meant to mark by the two other forms of irregular gout which he recognised, viz. the Retrocedent and the Misplaced. The former he defines "a gout, with inflammation of the joints suddenly receding, and *debility* of the stomach, or of some other internal part immediately succeeding;" the latter, as "gout with *inflammation* of some internal part, inflammation of the joints either having not preceded, or having preceded and quickly disappeared." It is quite obvious that in both of these forms, the internal affection is considered as constituting the important character of the disease; and that they are principally distinguished from one another by the different nature of this internal affection, which is characterised as *debility* in the one, and as *inflammation* in the other. Sir C. Scudamore combines under one form all those cases in which an external gouty inflammation suddenly disappears, and is succeeded by an internal af-

fection, of whatever nature that may be, whether spasmodic or inflammatory; and giving to this form the title of Retrocedent, he censures Dr Cullen for not having recognised, under what he termed retrocedent gout, the frequently inflammatory character of the internal affection, just as if Dr Cullen had made no mention whatsoever of a misplaced form of gout.

Whether, in the retrocession of external gout, and the occurrence of the disease in internal parts, the internal affections participate most frequently of a spasmodic or of an inflammatory nature, is a point upon which there may be room for much difference of opinion. Sir C. Scudamore believes that the internal inflammatory affections are much more frequent than the spasmodic. Dr Cullen, again, regarded the internal gouty affections as being usually of the spasmodic kind. That they are sometimes of the one character, and sometimes of the other, seems to be now universally acknowledged; and great obligations are certainly due to Dr Cullen for having pointed out this distinction so clearly as he has done, in the respective definitions which he has given of his retrocedent and his misplaced varieties of gout. Sir C. Scudamore, himself, in his observations on Dr Cullen's definition of misplaced gout, shews that he was aware of its comprehending one of the varieties of his retrocedent form. "The definition concludes," he remarks, "with a repetition of what was before said of the retrocedent variety, with the difference that the new internal action is here unequivocally designated as that of inflammation." Yet when he comes to speak of the treatment of retrocedent gout, he speaks of Dr



Cullen and authors in general, as appearing to have considered it as a settled axiom of practical doctrine, that debility and spasm, and not inflammatory action, seize the internal organ in the case of retrocedent gout; he alleges that in every case of retrocedent gout, Dr Cullen has directed a treatment entirely stimulating; and professes himself disposed to propose a very different general rule of practice, in opposition to that laid down by Dr Cullen. Now, the simple answer to all this is, that the rule of practice upon which Sir C. Scudamore so much insists, is that which Dr Cullen, in following Sydenham, laid down for what he called misplaced gout. Sydenham, in speaking of the risk of some violent symptom coming on suddenly, after the striking in of the gouty matter, mentions diarrhoea as one of these, and then proceeds—"There is another symptom which I have often seen, though it is not so common, viz. a translation of the peccant matter to the lungs, by a cough in the winter season, occasioned by taking cold in the fit, which by degrees invites the matter to those parts, the joints the mean while being in great part or totally freed from the pain and swelling, from the translation of the morbid matter to another part. In this single case the curative indication is not to be levelled at the gout, but this symptom is to be treated like a true peripneumony; namely, by repeated bleeding, and cooling and incrassating medicines and regimen, as the blood that is taken away, especially in this symptom, exactly resembles that of pleuritic persons."

Musgrave, in acknowledging the necessity of venesection in those forms of anomalous gout in which hæmoptoe and peripneumony present themselves, considers Sydenham as having gone too far in his recommendation of that plan of treatment. But Dr Cullen,

in his First Lines, stated that in the misplaced gout, the disease is to be treated by bloodletting, and by such other remedies as would be proper in an idiopathic inflammation of the same parts; and in his lectures he was accustomed to observe, "What I call the *misplaced* is more immediately connected with the regular gout; it is where an inflammatory state takes place, and that inflammation is not in the joints, but in some of the internal parts, whose functions are of more immediate importance. What I have to say with regard to it is, that as far as writers upon this subject have been clear and distinct, and I have been told that they are not so always, this case of misplaced gout is attended with the same symptoms as attend the idiopathic inflammation of the same viscera; and so we have the peripneumonia arthritica of Sydenham and Musgrave; and at the same time we learn that it is attended with all the symptoms of inflammatory peripneumony; and as this is generally and universally the case, I can discuss the matter by further adding, that they also agree in this, that it is to be treated by the same remedies, chiefly by venesection. I have given," he adds, "the difficulties which occur with regard to venesection in regular gout; perhaps somewhat of the same doubts might occur here, but we are not thus to deliberate, we are under the strongest temptation and necessity of hazarding all the consequences of bleeding with regard to the constitution in general, when life is immediately at stake."

The descriptions which Dr Cullen has given of the diseases comprehended under his third order of Pyrexiae, the Exanthemata, were, for the period at which they were written, uncommonly correct, and appear to have been all greatly improved by the results of his own observations and experience. The rules, also, which he has laid down with regard to the treatment

of these diseases, and in particular with regard to the use of bloodletting in Measles and Erysipelas, are extremely distinct, judicious, and important.

With respect to the supposed eruptive fever, which had been described under the name of Miliary Fever, and imagined to have appeared for the first time in Saxony about the middle of the 17th century, Dr Cullen, while he gives an account of what he found delivered by authors who had considered the white miliary fever as an idiopathic disease, declares, for his own part, "after having often observed the disease, I must say that I doubt much if it ever be such an idiopathic as has been supposed; and I suspect that there is much fallacy in what has been written on the subject." Dr Beerenbroeck, in writing to Dr Cullen, soon after the publication of the second volume of the First Lines, says, "I have lately sent a paper on the Miliary Fever to the Medical Society, which is to concur for the prize to be awarded next August. The question is, Whether there exists a Miliary Fever as a primary affection, and distinct from the other exanthemata? Your treatise on the Miliary Fever has been of great service to me, and I find your opinion confirmed in a book just published by the successor to the late Dr De Haen (Stoll), who proves, from observations made at Vienna, that it is not a primary and distinct disease; that it is not contagious," &c. And after transcribing a few lines from Stoll's *Ratio Medendi*, he adds, "I will make no other reflection upon the extract, than that most of his observations confirm what you have delivered in your treatise on the Miliary Fever." I have inserted in the Appendix

(Note I.) a letter in which an opposite view of the nature of Miliary Fever is adopted, that was addressed to Dr Cullen by one of his early and favourite pupils, Dr Francis Hutcheson,\* son of the celebrated professor of Moral Philosophy in the University of Glasgow, and who was at that time a distinguished medical practitioner in Dublin.

In recognising the Hæmorrhages as an order of the class Pyrexia, Dr Cullen, as we have already seen, included under this title only those cases of sanguineous effusion that are attended with some degree of pyrexia, that seem always to depend on an increased impetus of the blood in the vessels pouring it out, and that arise chiefly from an internal cause. From this order he excluded, therefore, not only those effusions of red blood that are owing entirely to external violence, but those also which, though arising from internal causes, are not attended with pyrexia, and which are seemingly owing to a preternatural fluidity of the blood, and to the weakness or erosion of the vessels, rather than to any increased impetus of the blood in them.

As an essential foundation for a general doctrine of Active Hæmorrhage, Dr Cullen endeavoured to mark the order in which the several phenomena of this state succeed each other, as consisting of, 1st, Those indicative of fulness and tension about the parts from whence the blood is to issue ; 2d, Those indicative of the state of Pyrexia, the cold stage followed by a

\* See Appendix to vol. i. p. 583.

hot stage, in the course of which last the blood bursts forth ; and, 3d, The spontaneous cessation of the effusion, and, along with it, of the hæmorrhage.

In the view of hæmorrhage taken by Stahl, the effusion of blood was regarded as the immediate and necessary consequence of a congestion of blood in a particular part, which congestion was in its turn attributed to motions excited, in a plethoric system, by the Soul for its own wise purposes. Hoffmann, in his explanation of active hæmorrhages, had interposed the occurrence of febrile action between the states of simple congestion and of hæmorrhage ; and in this view he was followed by Cullen. Dr Cullen endeavoured, however, if not to trace, at least to lay a foundation for tracing, the sequence of events between the occurrence of congestion in a particular part, and the production of fever, and its attendant increased action of vessels, by a reference to the *vires medicatrices Naturæ*.

To explain the production of that unequal distribution of blood in different parts of the body, out of which local congestions arise, and which, as has been seen, was attributed by Stahl to tonic motions excited by the Soul, Hoffmann availed himself of the doctrine of constriction of the extreme vessels, which he had applied to fever, and arrived at the conclusion that fever and hæmorrhage are produced by the same motions ; the only difference being, that in fever these motions are more, and in hæmorrhage less complete. Dr Cullen did not limit himself to constriction of the extreme vessels as the only morbid condition which may form the first link of the proximate cause of hæmorrhage ;

but conceived that the remote causes which give rise eventually to hæmorrhagic effusions, may operate primarily on the vascular system, either by producing increased contraction or increased distention.

An important part of the general doctrine of hæmorrhage, on the explanation of which Dr Cullen bestowed much pains, is its liability to frequent recurrence. This circumstance he endeavoured to explain upon two principles; *first*, That though the flowing of the blood relieves for the time the existing congestion and consequent irritation, yet the internal causes by which the unequal distribution of blood was originally produced, commonly remain, and must now operate the more readily, as the over-stretched and relaxed vessels of the part will more easily admit of a congestion of blood in them; and, *second*, That hæmorrhage has always a tendency to increase the general plethoric state of the system, which state renders every cause of unequal distribution of blood of more considerable effect than it would otherwise be. The tendency of hæmorrhage to increase plethora, Dr Cullen supposed to depend on the ordinary excretions being suspended by the hæmorrhagic effusion, and not restored in time to counteract the renewal and further accumulation of blood.

But the part of the general doctrine of hæmorrhage, on the elucidation of which Dr Cullen seems to have bestowed the largest share of attention, was the liability of hæmorrhages to happen in certain parts of the body more frequently than in others, and at certain periods of life more readily than at others; why epistaxis, or hæmorrhage of the nose, should be so fre-

quent before the period of puberty ; why hæmoptysis, or hæmorrhage of the lungs, should especially occur about the time of the body's arriving at its acmé ; and why the hæmorrhoidal flux and hæmorrhage of the brain should happen principally to men in the decline of life. In each of these cases, he conceived that the hæmorrhage depends on a disturbance in the balance between the several parts of the circulation, but that, whilst during the period of puberty, this disturbed balance is between the several parts of the general circulation as subservient to the growth of the different parts of the body, in the acmé of life it is the balance between the general and the pulmonic circulation that is most liable to be disturbed, and in advanced life, again, the balance between the arterial and the venous circulation.

With respect to the treatment of hæmorrhagic diseases, Dr Cullen was by no means disposed to accede to the favourite doctrine of Stahl and his followers, that they should be left to the conduct of nature. He readily admitted that the human body, upon many occasions, becomes preternaturally plethoric ; that the dangerous consequences which might be apprehended from such a state, seem to be obviated by a spontaneous hæmorrhagy taking place ; and that the suppression of a hæmorrhage, under such circumstances, may occasion many disorders. But believing it to be possible to prevent or remove a plethoric state by other means besides that of spontaneous hæmorrhage, Dr Cullen was of opinion that such hæmorrhage is to be avoided, *1st*, because it does not always happen in parts where it is safe ; *2d*, because, while it does relieve a ple-

thoric state, it may, at the same time, induce a very dangerous disease ; 3d, because it may often go to excess, and either endanger life or induce a dangerous infirmity ; and, *lastly*, because it has a tendency to increase the plethoric state it was meant to relieve, to occasion its own recurrence, and thereby to induce a habit which, if left to the precarious and unequal operation of nature, may, from the frequent errors of this, be attended with much danger. With respect to those hæmorrhages which do not arise from necessities of the system, but proceed from incidental causes, he was satisfied that they may be immediately suppressed, and that the repetition of them, as tending to induce a plethora, and a habit not otherwise necessary, may be prevented, with great advantage ; and, on the whole, he concluded that every preternatural hæmorrhage is to be avoided, and especially the returns of it to be, if possible, prevented.

The place assigned by Dr Cullen, in his Nosology, to Phthisis Pulmonalis, that, viz. of a sequel to Hæmoptysis, has been the subject of much criticism, and has even been referred to as shewing how little Dr Cullen was acquainted with the pathology of this disease. But however objectionable this collocation may be considered, Dr Cullen's description of pulmonary consumption shews that he had bestowed on this disease much anxious and long-continued investigation, both in the living subject and in the dead body. In his Nosology, he defined it as "emaciation of the body and weakness, with cough, hectic fever, and, for the most part, purulent expectoration ;" and, in his First Lines, more shortly as "an expectoration of pus or purulent mat-



ter, with a hectic fever." Being persuaded that in every instance of an expectoration of pus, there must exist an ulceration of the lungs, he concluded that in the cases in which such an expectoration has been supposed to take place independently of ulceration of the lungs, the expectoration must have been actually of a mucous character. He conceived that there are several different previous affections of the lungs from which phthisis pulmonalis, as consisting of an ulcer of these organs accompanied with hectic fever, may arise, viz. 1st, Hæmoptysis; 2d, Suppuration of the lungs in consequence of Pneumonia; 3d, Catarrh; 4th, Asthma; and, 5th, Tubercles, the last of which he regarded as the most frequent of the causes of Phthisis; indeed, he assigns his reasons for believing that in all cases in which phthisis succeeds to inflammation, to hæmorrhage, or to catarrh, either other causes concur to produce tubercles, or these morbid states themselves act as causes of tubercles, and, consequently, that tubercles containing a matter not fit to be changed into pus, are the chief foundation of phthisis. In reference to the connection between phthisis and hæmoptysis, in particular, he was accustomed to observe in his lectures, that we meet with many cases of tubercles of the lungs where the disease had not been preceded by any spitting of blood, and to profess himself disposed to believe that "perhaps hardly any spitting of blood does prove the forerunner of phthisis, unless where the spitting of blood is connected with, and probably produced by, tubercles." Dr Cullen was accustomed also to point out the connection that seems to exist between the

scrofulous habit and tubercular phthisis. "Frequent dissections in cases of phthisis," he says, "have shewn that the lungs are affected with tubercles, little, round, or variously formed nodules putting on the appearance of glands, and which, from many circumstances, have appeared to be lymphatic glands thus tumefied." He does not seem, however, to have been himself satisfied with this explanation of the nature of tubercles. "I must own," says he, "that in many cases it can hardly be supposed that the number of tubercles which we find in the whole cellular texture of the lungs, can all be tumefied glands."

Dr Cullen was strongly impressed with the persuasion that the progress of tubercular phthisis is intimately connected with inflammatory action, and the plan of treatment which he recommended was framed in consonance with this view. With respect to the cure of tubercles, he observes in his First Lines,—“All that at present seems to be within the reach of our art, is to take the measures proper for avoiding the inflammation of tubercles. It is probable that tubercles may subsist long without producing any disorder; and I am disposed to think, that nature sometimes resolves and discusses tubercles which have been formed, but that nature does this only when the tubercles remain in an uninflamed state; and, therefore, that the measures necessary to be taken, are chiefly those for avoiding the inflammation of the tubercles.”

To those who know any thing of the recent progress of morbid anatomy, in this country and on the Continents of Europe and America, it is unnecessary to say how much labour has been bestowed since the time of Dr Cullen, in prosecuting the natural history of Tubercle, both in the lungs and in other textures and

organs of the body ; but it is not undeserving of notice here, that the first person who treated of this species of morbid deposition as a subject of separate and peculiar investigation, was an esteemed pupil of Dr Cullen's, viz. Dr William Stark. Of this ingenious physician, who, at an early age, fell a victim to his zeal for experimental inquiry,—his death having been occasioned by a course of experiments, made upon himself, on the effects of various kinds of food,—it is mentioned by his friend and biographer, Sir J. C. Smyth, that “from Glasgow he repaired to Edinburgh, where he was soon distinguished and honoured with the friendship of Dr Cullen, who is not more eminently conspicuous for the superiority of his own genius, than quick-sighted in distinguishing, and liberal in encouraging it in others.”

In the institution of his class *Neuroses*, Dr Cullen proceeded on the persuasion that the nervous system exercises an important influence in regulating the performance of all the three Galenical divisions of the corporeal functions,—the Animal, the Vital, and the Natural,—as well as of those of the Mind ; and, accordingly, he included under this nosological class, diseases referrible to each of these divisions of functions.

Of the diseases of the animal functions included under the *Neuroses*, and which, by his own statement, this class had chiefly in view, some found their place under the order *Comata*, viz. *Apoplexy* and *Palsy* ; and others under the order *Spasmi*, viz. *Tetanus* and *Epilepsy* as *Tonic*, and *Chorea* and *Hysteria* as *Clonic*, spasmodic affections.

*Apoplexy* was considered by Dr Cullen to depend

on an interruption of the motions of the nervous power, which interruption, he conceived, may be occasioned either, *first*, by morbid conditions producing a very general compression of the brain as the origin of the nerves ; or, *second*, by causes destroying directly, and independently of compression, the mobility of the nervous power.

The most frequent causes of cerebral compression he considered to be vascular distension and effusion, the latter of which, he supposed, may consist either of blood or of serum. The states both of over-distension and of effusion may be produced, he farther conceived, by whatever increases the afflux and impetus of the blood in the arteries of the head ; but they are more frequently produced by causes which operate by preventing the free return of the venous blood from the vessels of the head to the right ventricle of the heart.

In employing the doctrine of cerebral compression to explain the phenomena of apoplexy, Dr Cullen seems to have been aware that this doctrine is not unattended by physical difficulties. "The cranium," he remarked in his lectures (Works, vol. ii. p. 339), "is very accurately fitted to its contents ; and any turgescence of the cerebral vessels must certainly be limited by it. If the bloodvessels are, therefore, upon any occasion, preternaturally distended, they must occupy the place allotted to them and to the medullary substance. I would, therefore, not consider the state as a compression on the origin of the nerves, but rather as a general dilatation of the whole cortical substance and of the vessels interposed in it, operating by a compression on the whole medullary substance." It may be doubted, however, whether, by any restriction of the nature here suggested, the doctrine of cerebral

compression could be reconciled with the actual physical conditions of the encephalon, as suggested by Dr Monro secundus, and more fully established by the recent experiments of Drs Sanders and Seeds and of Dr Kellie, and by the reasonings of Drs Carson, Abercrombie, Serres, and others. From these it would appear, that, as the substance of the brain is nearly incompressible, as the parietes of the cranium are unyielding, and as the atmospheric pressure can, so long as the cranium is entire, act on the encephalic bloodvessels only in one direction, the absolute amount of the contents of this cavity cannot undergo any change in health or in disease, whatever variations may occur in the relative proportions of the several substances, solid and fluid, of which these contents consist. No portion of these contents, therefore, can be withdrawn, without its place being simultaneously occupied by some equivalent; nor can any thing exuberant be intruded, without an equivalent displacement. It is these physical difficulties in the doctrine of cerebral compression,—which do not seem to be, as yet, everywhere fully appreciated,—that has rendered it necessary to seek for some other explanation of the changes which occur, in apoplectic affections, in the exercise of the animal and intellectual functions. How far such an explanation may be found in the varying rapidity with which the blood may circulate through the cavity of the cranium, or in the variations that may take place in the relative proportion of this fluid contained in the several sets of encephalic vessels, the arteries, the capillaries, the veins, and the sinuses, or in the several portions of each of them, are questions still open for consideration. M. Magendie

has satisfied himself that there naturally exists, within the cranium and spinal canal, a certain quantity of serous fluid capable of passing into the spinal canal, in the event of an accumulation of blood occurring, from any cause, within the cranium, and of passing back again into the cranium on the cessation of such vascular turgescence. This ingenious speculation would, if duly corroborated, afford a simple explanation, consistent with the physical principle above adverted to, of those cases of general vascular turgescence of the encephalon, —occurring, as we have reason to believe, in a sudden manner, and without any possibility of a proportional wasting of the solid substance of the brain,—which from time to time present themselves to our observation.

In conceiving, from the results of post-mortem examinations, that there is a foundation for the common distinction of apoplexy from compression into the Sanguineous and the Serous, Dr Cullen was satisfied that this distinction cannot be very usefully applied to practice, as both kinds may often depend on the same cause, —that is, a venous plethora, and therefore require very nearly the same method of cure. In more recent times, Dr Abercrombie has called in question the existence of any modification of apoplexy that can properly be called Serous ; contending, in the *first* place, conformably with Dr Cullen's view, that there is no foundation for the distinction between sanguineous and serous apoplexy, in the persons attacked, in the symptoms exhibited, or in the mode of treatment required ; *second*, Dr Abercrombie considers it as in the highest degree improbable, on pathological grounds, that serous effusion should occur in the brain as a primary

disease, and accumulate with such rapidity as to produce the symptoms of an apoplectic attack ; and *third*, he contends, that where serous fluid is found effused, its quantity bears no proportion to the degree of the apoplectic symptoms, and its presence, consequently, cannot be considered as the cause of these symptoms. Dr Abercrombie insists very strenuously and ably on the existence of a form of apoplexy which occurs, independently of either sanguineous or serous effusion, and in which, when death occurs, we cannot, on inspection, discover in the brain any satisfactory deviation from the healthy structure. This affection he proposes to call *simple* apoplexy ; and he conceives that many of those cases of apoplexy terminating fatally, in which serous effusion has been met with to a greater or less extent, were really simple in their origin, and that this effusion is to be regarded as having supervened in the course of the disease, and not as having been its primary cause.

Dr Cullen, as we have seen (p. 193), recognised the possibility of apoplexy occurring independently of any of those morbid conditions which he conceived capable of occasioning compression of the brain, supposing, as he observes, that the mobility of the nervous power may, by the agency of certain morbid causes, be directly destroyed ; or, in the language of Dr Abercrombie, he admitted a form of simple apoplexy. He was well aware, also, of the existence of several sources of fallacy, in judging from the appearances that present themselves on the dissection of persons dying of apoplexy, as to the actual cause of the disease. In particular, he notices, in his lectures, that, in some cases in

which morbid substances, solid or fluid, are found within the cranium, these are of too inconsiderable bulk to have produced general compression of the brain ; and he was consequently led to suspect that a partial compression is sufficient to produce a pretty general collapse over the whole medullary substance. In other cases, as he has pointed out in his First Lines, though the foreign matters found within the cranium are of considerable amount, so that the appearances upon dissection might lead us to think that the apoplexy had depended entirely upon compression, there is reason to believe that these foreign matters have been developed in the progress of the apoplexy rather than operated as its cause. "Whatever," says he, "takes off or diminishes the mobility of the nervous power, may very much retard the motion of the blood in the vessels of the brain, and that, perhaps, to the degree of increasing exhalation or even of occasioning rupture and effusion ; so that, in such cases, the marks of compression may appear on dissection, though the disease had truly depended on causes destroying the mobility of the nervous power."

In his account of the Chorea or Dance of St Vitus, Dr Cullen remarks (§ 1353) that, "as in this disease there seem to be propensities to motion, so various fits of leaping and running occur in the persons affected ; and there have been instances of this disease, consisting of such convulsive motions, appearing as an epidemic in a certain corner of this country." I find, among Dr Cullen's papers, a letter addressed to him by Dr Farquharson, then of Dundee and afterwards of Edinburgh, which he had probably in view in this



remark. The Louping or Leaping Ague of Forfar, of which Dr Farquharson's letter contains an account, has since been described by several of the contributors to the Statistical Account of Scotland, but by none more graphically than by that gentleman himself, whose letter I have therefore inserted in the Appendix (Note K). It may be remarked, however, that the propriety of referring this imitative convulsive affection to chorea seems very doubtful ; and that it is probably more nearly akin to hysteria.

Dr Cullen did not introduce Headache in any of its forms (Cephalæa, Cephalalgia, Hemicrania), either into his Nosology or into his First Lines ; but placed them in the list of Omissi subjoined to the former. The omission was perhaps attributable, in part, to his experiencing difficulty in finding a place for this affection in his systematic arrangement, and, in part, to his regarding it as a symptom of other diseases, rather than as constituting a particular disease in itself. In order to supply this omission, I have, in my edition of Dr Cullen's Works, introduced a series of clinical lectures on Headache, from the notes of the course delivered by him in 1765-6, that were published after his death from the manuscripts of a pupil. These lectures, if not marked by the same systematic unity, at least evince the same practical acquaintance with disease, and the same nicety of diagnostical discrimination, that characterize the descriptions of other nervous affections contained in his authorized publications.

The very full statement that has been given in the first volume of this work (pp. 264 and 343), of Dr Cullen's views respecting the dependence of both the

healthy and the morbid phenomena of sensation and motion on the nervous system, renders it unnecessary to enter here at any greater length on the explanations which he proposed of the particular diseases of the animal functions ; the more so that these do not appear to have been subjected to nearly so much animadversion as his accounts of the diseases belonging to the several orders of the class *Pyrexiaë*. The labour bestowed, since Dr Cullen's time, on the investigation of the pathological conditions of the several portions of the nervous system presiding over the animal functions, has been immense, and much additional information respecting these conditions has unquestionably been accumulated. But though this be true, Dr Cullen's descriptions of the principal diseases to which these functions are subject, and the methods of treatment which he recommends for each of them, must still be regarded as accurate and judicious.

The treatises of Abercrombie, Andral, Lallemand, Bright, and Van Coëtsem, on the Diseases of the Brain and its Membranes generally, that of Parent-Duchatelet and Martinet on Inflammation of the Arachnoid Membrane, that of Bayle on Chronic Meningitis, that of Bouillaud on Inflammation of the Brain, that of Rostan upon Softening of the Brain, those of Coindet, Golis, Charpentier, Cheyne, and Davis on Hydrocephalus, those of Portal, Cheyne, Rochoux, Serres, and Richond on Apoplexy, those of Ollivier on the Spinal Cord, of Brown, Teale, Mammers, Griffin, Darwall on Spinal Irritation, and of Swan and Descot on the Nerves,—these, and a host of other works which have appeared in recent times, taken in

connection with the physiological investigations into the dependence of the sensory and motory functions on the several parts of the nervous system, that have been prosecuted during the same period by Bell, Bellingeri, Magendie, Mayo, Marshall Hall, Rolando, and Flourens, give a promise of an enlarged and comprehensive view of the influence of the nervous system in the diseases of the animal functions, which Dr Cullen must have contemplated with the utmost satisfaction. It is only to be wished that some master-spirit like his would occupy itself in endeavouring to reduce these various and complicated results of observation and inquiry into a few simple and general doctrines.

Intimately connected with the diseases which consist in derangements of the sensory and motory powers of the nervous system, must be considered those in which the Intellective powers are affected. To the consideration of the diseases of the mind—the *Vesaniæ*—in all their relations, theoretical and practical, Dr Cullen brought a thorough acquaintance with those analytical investigations in mental philosophy, for the prosecution of which his countrymen are known to have long entertained a strong predilection, and in the cultivation of which several of his more intimate friends and associates had particularly distinguished themselves. In the French criticism on the First Lines already referred to, Dr Cullen's view of the *Vesaniæ* is considered as the part of the work in which his sagacity and the clearness of his ideas appear to most advantage.

We have seen (vol. i. p. 356) that in his Lectures on Pathology, Dr Cullen divided the morbid conditions

of the intellectual faculties under the two heads of Imbecility or Fatuity, and Error or Delirium ; the former consisting in weakness or imperfection of thought, and the latter in its false exercise. Considering the three principal operations of thought to be Perception, Judgment, and Will, Dr Cullen adopted the three modifications of delirium pointed out by Gaubius, which correspond with false perception, erroneous judgment, and irregular passions. In his First Lines, accordingly, he defined delirium to be “ in a person awake, false judgments arising from perceptions of imagination or from false recollections, and commonly producing disproportionate emotions.”

In endeavouring to establish a general theory of Delirium, Dr Cullen had recourse to his doctrine of the nervous power being at different times in different degrees of mobility and force, or of those states which he termed excitement and collapse. (Vol. i. p. 311.) For reasons which he has stated in his First Lines, he was disposed to think that not only can the different states of excitement and collapse take place in different degrees in the brain generally, at different times, but that they can take place in different degrees at the same time in the several parts of that organ, or at least with respect to the exercise of the different functions ; and from a review of the phenomena of sleep and delirium, he considered it as sufficiently evident that delirium may be, and frequently is, occasioned by such an inequality in the excitement of the brain.

Dr Cullen recognised two kinds of Delirium, one in which it is combined with pyrexia and comatose affections ; the other in which it is entirely without any such

combination, the latter being the form of delirium which is properly named Insanity. In referring to the attempts which Dr Arnold had made to distinguish the different species of insanity, Dr Cullen deemed it advisable to confine himself to the two heads of Mania and Melancholia, confessing that though he was sensible that these two genera do not comprehend the whole of the species of insanity, he was not clear in assigning the species which may be comprehended under each of them. In his Synopsis of Nosology he had employed the terms Mania and Melancholia as synonymous with general and partial insanity, or what has since been called Monomania; but in his First Lines he expresses himself as inclined to believe that the limits between general and partial insanity, cannot always be so exactly assigned as to determine when the partial affection of the mind is to be considered as giving a peculiar species of disease, different from a more general insanity.

Since Dr Cullen's time, the consideration of the various forms of Insanity has been prosecuted with great assiduity; and a most important revolution has been effected in the system on which the management of persons labouring under mental derangement is now generally conducted. The circumstances which led to this happy change cannot be indifferent to any humane and intelligent mind. To the late M. Pinel is justly due the merit of having led the way in this benevolent reformation. His attention is supposed to have been first particularly directed to the subject in 1785, by the melancholy fate of a young man whom he greatly esteemed, who, in a state of insanity, escaped

from his father's house into a wood in the neighbourhood, where he was destroyed by wolves.\* About the time mentioned, M. Pinel became connected with an institution then forming for the reception of insane patients, and in it he is believed to have made the first trials of that milder method of conducting the treatment of the insane, which, as the author of his *Eloge* justly says, will for ever render his name dear to posterity. In 1791, the Royal Society of Medicine having proposed as the subject of a prize essay, "To point out the most efficacious means of treating patients whose minds have become alienated before senile age," M. Pinel presented a dissertation for competition, but whether successfully or not, does not appear. One of the members of the committee for the examination of the prize essays was the celebrated academician M. Thouret, who was about that time placed along with MM. Cousin and Cabanis at the head of the administration of the hospitals of Paris. Being greatly shocked at the condition in which they found the part of the Bicêtre Hospital appropriated to the reception of insane persons, they resolved on appointing M. Pinel physician to the establishment, in the conviction that no one could be found better qualified than him to introduce those reforms which were so loudly called for. On this office M. Pinel entered towards the end of 1792, and the beneficial effects of his milder plan of treatment speedily became manifest. Some years afterwards he was appointed physician to the Salpêtrière Hospital, to which the treatment of

\* See *Eloge* by M. Pariset, *Mem. de l'Acad. de Medec.* i. 199.

insane females was transferred from the Hôtel Dieu, and committed to his charge. In 1801 he published the results of his observations in this class of diseases ; and in 1809 his work appeared in an extended form under the title of a Medico-Philosophical Treatise on Mental Alienation.

In the fifth volume of the Memoirs of the Royal Academy of Medicine, there is published an excerpt from a historical notice of M. Pinel by his son, which contains a very striking narrative of his proceedings relative to the abolition of chains among the insane at the Bicêtre Hospital in 1792, derived from his own notes. In the perusal of that narrative, it is impossible not to feel the effect of the contrast between this act of twice-blessed mercy, and the horrors of which the adjoining capital was at that time the scene.

It is not a little interesting to find that, at the very time when, in France, MM. Cousin, Thouret, and Cabanis were engaged in taking the measures necessary for improving the system of treating the insane at the Bicêtre Hospital, in England the Society of Friends were engaged at York in similar considerations. And here again, it seems to have been from an accidental circumstance that results of so important a character had their origin. A female having died in a mad-house in the vicinity of the city of York, under circumstances calculated to give dissatisfaction to the Society of Friends, of which she was a member, it was suggested to Mr William Tuke that an institution for the reception of insane persons belonging to that society, and conducted by members of it, might prove highly beneficial. By that estimable philanthropist the sub-

ject was brought before the quarterly meeting of the Society at York in March 1792, and though considerable difficulties at first presented themselves, these were sufficiently overcome to admit of the opening of the Retreat, near York, in 1796. In 1813, Mr Samuel Tuke published a "Description of the Retreat, an institution near York, for Insane Persons of the Society of Friends, containing an account of its origin and progress, the Modes of Treatment, and a Statement of Cases." The striking contrast which that work presented of the measures which were found at once sufficient to maintain the discipline of the establishment, and most conducive to the bodily health, and mental tranquillity and improvement of the patients, with what was going on, at that very time, in other institutions for the insane, some of them in the immediate vicinity of the Retreat, could not fail to arouse attention to the subject; and the appointment of a Committee of the House of Commons, in 1815, of which the Right Hon. George Rose officiated as chairman, elicited so extensive and valuable a mass of information relative to mad-houses and the treatment of the insane in them, and presented this information to the public in so authoritative a form, that it could not fail to make a deep and beneficial impression.

Since that time the work of improvement has gone on rapidly, not only in France and Great Britain, but also in the other nations of Europe as well as in America; confinement of the insane in solitary cells has been in a great measure abandoned; and the various instruments of constraint, punishment, and terror, which used formerly to be regarded as indispensable



elements in the management of the insane, are now, with some very trivial exceptions, very generally laid aside, and happily replaced by means of occupation and amusement.

It is impossible to have contemplated this milder system, as it may be seen in operation in the hospitals for the insane at Bicêtre near Paris, at Siegburg near Bonn, at Heidelberg, at Pirna near Dresden, at Aversa near Naples, at Fort Clarence near Chatham, at Hanwell in Middlesex, and in various other institutions, both of a public and private nature, in this country and on the continent of Europe, without being struck with the wonderful difference of the effects produced on this class of patients by occupation, as far as they are capable of it, in in-door and out-door labours, from those occasioned and kept up by the harsh and often cruel measures formerly pursued, and not yet everywhere abolished.

The writings of Crichton, Haslam, Crowther, Burrows, Willis, Conolly, and Pritchard, in this country; of Foderé, Dubuisson, Esquirol, Ferrus,orget, Falret, and Foville, in France; of Reil, Heinrich, Friedreich, and Jacopi, in Germany; of Guislain in Belgium, &c. &c. fully attest that the philosophical investigation of these diseases in respect of their causes and phenomena, and of the structural alterations with which they are connected, has not been neglected amidst the enthusiasm that has been excited by the gradual discovery of the more and more extended applicability of the humane and moral system of treatment to the different classes of insane persons. Whatever opinions may be entertained respecting the

solidity of the particular doctrines of Phrenology, as distinguished from those of Metaphysics and Physiology taught before the time of Drs Gall and Spurzheim, it would be unjust not to allow that the attention which the writings and lectures of these physicians and of their followers has procured for psychico-physiological inquiries, has considerably assisted in increasing the public interest in questions connected with mental aberration.

The diseases of the organs subservient to the Vital functions, the Respiratory and the Circulatory, which Dr Cullen included under his class Neuroses, were, besides Chincough (which depends on a specific contagion, and is a disease of limited duration), Dyspnœa, Asthma, Syncope or Fainting, and Palpitation. Of the frequent influence of various morbid conditions of the Nervous System in producing these several forms of disease, no doubt can be entertained, whatever may be supposed to be the precise nature of the dependence of the organs in which they are seated, the Lungs and the Heart, upon the several portions of the Nervous System. And so far, therefore, their position among the Neuroses may be defended. But in a great many instances of Dyspnœa, Asthma, Syncope, and Palpitation, it is not simple functional derangements but serious structural alterations of the cardiac and pulmonary organs, that constitute the morbid or pathological conditions on which the external symptoms depend, or at least that constitute an important part of these pathological conditions. Accordingly, it was necessary for Dr Cullen to include under the genera of diseases that have been mentioned, all the structural

alterations of the Lungs and Heart, with the exception of their inflammations and of Hæmoptysis and Phthisis, his system presenting no other heads to which they could be referred.

To no department of scientific medicine, perhaps, have more important additions been made, since Dr Cullen's time, than to that which relates to the diseases of the Heart and Respiratory organs. For, besides the more accurate knowledge that has been obtained of the various structural alterations to which they are subject, and of the connection of these several alterations with particular symptoms or combinations of symptoms, resulting from the morbid exercise of the functions of these organs themselves, or of organs with which they are related, their diagnosis has been still further promoted by the discovery of a series of phenomena of a physical character, those, namely, of Percussion and Auscultation, by which the recognition of the existence of these alterations, and their discrimination, are very considerably facilitated. The writings of Bayle, Laennec, Andral, and Stokes, on the Diseases of the Respiratory Organs generally, and of Louis, Clark, and Morton, on Tubercular Phthisis, and those of Corvisart, Burns, Laennec, Testa, Kreyssig, Bertin, Hope, Bouillaud, and Pigeaux, on the Diseases of the Heart, have familiarized practitioners with a number of morbid affections which previously were either not known to exist, or which were known only as subjects of anatomical investigation.

Nearly the same observations which have just been made in respect of the diseases of the Vital functions, are applicable to those of the Natural functions com-

prehended by Dr Cullen under his class *Neuroses*, viz. *Dyspepsia* and *Pyrosis*, for the Stomach ; *Colic*, *Cholera*, and *Diarrhœa*, for the Bowels ; and *Diabetes* for the Urinary organs. Whatever share the Nervous system may have in producing affections which admit of being included under these nosological genera, it must be acknowledged that in several of the species of some of them at least, it is on structural alterations of the organs concerned, and in some, perhaps, on morbid conditions of the blood, rather than on derangements of the nervous system, that the symptoms chiefly depend. And it is only, therefore, in courses of lectures or in treatises, in which all the diseases of the same organs are brought together, and considered in respect of the morbid conditions, functional and structural, on which they depend, of the remote causes by which they are produced, of the symptoms by which their existence is manifested, and of the means of treatment which may be most advantageously employed for their relief or removal, that a comprehensive and proper view of such affections can now be taken.

In respect of *Diabetes*, it is not undeserving of notice, in reference to more recent views of the pathology of this disease, that Dr Cullen was satisfied that its production is nowise dependent on any topical affection of the kidneys, but is rather to be imputed to a fault in the assimilation of the fluids (§ 1510). “ I think it probable,” he remarks (§ 1511), “ that in most cases, the proximate cause of this disease is some fault in the assimilatory powers, or in those employed in converting alimentary matters into the proper animal fluids. This I formerly hinted to Dr Dobson, and it

has been prosecuted and published by him ; but I must own that it is a theory embarrassed with some difficulties which I cannot at present very well remove." The publication by Dr Dobson here referred to, appeared in the 5th vol. of the Medical Observations and Inquiries, p. 298, and is dated Liverpool 20th Nov. 1774. I have already given (Appendix to vol. i. p. 625) the introductory part of a criticism on Dr Dobson's paper, written by Dr Cullen, to whom the manuscript had been submitted previously to publication. In a subsequent part of the same criticism, Dr Cullen states, nearly in the words afterwards employed by Dr Dobson, the considerations which led him to think that in cases of Mellitic Diabetes, " the disease depends especially upon a defect in the assimilatory powers of sanguification." The accuracy of Dr Dobson's conclusions respecting the saccharine character of the blood in the diabetic patient who had fallen under his observation,—on which Dr Cullen mainly rested his idea of diabetes consisting in imperfect assimilation,—has since been frequently called in question, Dr Francis Home, Nicolas and Gueudeville, Wollaston, Kane, and several other eminent chemists having all failed in detecting sugar in the serum of the blood of persons labouring under this disease. The more recent experiments, however, of Ambrosioni, Maitland, and Macgregor, seem to leave little room for doubt as to the fact of diabetic blood containing sugar, and must, consequently, be regarded as corroborating very powerfully Dr Cullen's doctrine respecting the proximate cause of diabetes. Mr Macgregor's experiments seem also to establish the existence of sugar, in considerable

quantity, in the stomachs of diabetic patients, particularly whilst they are in the use of a vegetable diet. It does not seem, however, to be as yet determined among the higher authorities in animal chemistry, whether the presence of sugar in the stomach,—which, it may be observed, seems even to occur, though to a less amount, in the state of health,—be attributable to its formation there by processes of combination or of decomposition, or whether it be owing, as Dr Cullen supposed, to the saccharine constituents of the food not undergoing their ordinary assimilation or conversion into other proximate animal principles, as albumen, &c.; whether, in short, it be owing to preternatural or to deficient assimilation. Dr Prout, while he differs from Dr Cullen's view of the proximate cause of diabetes, in believing it to lie partly in the kidneys, has been led by the general conclusions at which he has arrived relative to the assimilative processes going on in the animal economy, to entertain opinions respecting the agency of the stomach in the production of diabetes, very analogous to those which Dr Cullen had formed. “In diabetic affections,” Dr Prout remarks, “the reducing function of the stomach seems, in some instances, to be almost morbidly active; and farinaceous (and even other) matters are reduced to the condition of low saccharine matter, which the converting function of the stomach is incapable, as in health, of changing into the elements of chyle or blood. The consequence is, that this reduced or dissolved saccharine matter is taken up with the little chyle that may be formed, and after producing various derangements in its transit through the system, is ejected with the urine.”

The class Cachexiæ, and its several orders, Marcorres, Intumescentiæ, and Impetigines, do not contain many diseases, in respect of which Dr Cullen entertained peculiar opinions. The various pathological conditions, however, from which Dropsical effusion may arise, was a subject upon which he bestowed much consideration. I find among his papers a printed table of the several causes of dropsy, which he seems to have been in the use of placing in the hands of his students previously to the publication of his First Lines, as being, from its comprehensive nature and systematic arrangement, calculated to be of use to them. (See Appendix, Note L.)

The first circumstance in this table that must strike the pathologist acquainted with the views recently promulgated respecting the pathological conditions on which dropsy depends, is the prominent place assigned in it to resistance to the return of the blood, by the veins, to the heart, as a cause of increased serous exhalation, whether that resistance be occasioned by obstructions in certain thoracic and abdominal viscera, by obstruction of particular veins from causes seated within their cavities, or in their coats, or exterior to them, by the posture of the body, or by a general venous plethora. In recent times, M. Bouillaud has taken great credit to himself for having, on the strength of numerous observations, asserted that all the dropsies called Passive occur and are formed under the influence of some obstacle to the venous circulation; and for having shewn that, in a very large number of cases, this obstacle consists in the obliteration of the veins by concretions of blood, of longer or

shorter standing. This opinion, M. Bouillaud alleges, was very different from that generally taught at the time; conformably with which, passive dropsies were attributed to a general debility, manifesting itself, in the first place, in the lower extremities; and to atony of the lymphatic vessels. Dr Cullen has remarked, in his *First Lines*, that “the ordinary exhalation may be increased by various causes, and particularly by an interruption given to the free return of the venous blood from the extreme vessels of the body to the right ventricle of the heart.” And after shewing that this interruption may be owing to certain circumstances affecting the course of the venous blood generally,—diseases, namely, of the heart and lungs,—he adds: “But, further, the interruption of the blood in *particular* veins may likewise have the effect of increasing exhalation and producing dropsy;” in illustration of which, he alludes to obstructions of the Vena Portæ from diseases of the liver; and he further adds, that, “even in smaller portions of the venous system, the interruption of the motion of the blood in particular veins has had the same effect.” When, therefore, M. Bouillaud says that, to refute in part the old theory of passive dropsies, it was sufficient for him to prove that there occur partial dropsies, confined to one or other limb or to one or other cavity, seeing that dropsy could not possibly localize itself in this way if it were really produced by general debility, he is contending against a doctrine which Dr Cullen, at least, certainly did not entertain in the exclusive form in which M. Bouillaud seems to suppose it to have been entertained by medical men generally. These remarks are not intended to detract from



the merit and importance of the numerous and excellent illustrations which M. Bouillaud has brought forward of the frequent dependence of partial dropsies on the obliteration of particular veins ; but merely to shew that dropsy, dependent on venous obstruction,—and that obstruction arising from concretions within the veins,—had by no means been overlooked by Dr Cullen.

A second circumstance deserving of notice in Dr Cullen's Table is, that, besides regarding a general plethora in the veins as capable of occasioning serous effusion, by resisting the return of the blood to the heart, he has set down, as a distinct cause of increased exhalation, increased impetus of the blood in the arteries, occurring as a consequence of external violence, as in sprains and bruises, or as a consequence of fever ; in short, that he recognises the existence of what are now termed Active dropsies. "Independently of external violence, and merely from increased impetus of the arteries themselves," said he in his lectures, "an effusion may take place. There hardly occurs any inflammation of the abdomen, &c. that is not attended with more or less of an effusion from the surface into the cavities ; so the species of the Pleuritis Hydrothoracica of Sauvages is well founded, and is ascertained by dissection. We had a case of this very lately in one of our patients in the Infirmary." It is singular that Dr Cullen should have omitted, in his First Lines, to make mention of this cause of increased serous exhalation. The frequent connection of dropsical effusion with a plethoric, sthenic, or even inflammatory condition of the sanguiferous

system, has been much insisted on since Dr Cullen's time in the writings of Grapengiesser, of Dupuytren and Breschet; of Blackall (who very candidly mentions that suspicions of an inflammatory state in dropsy are not new, and that authors have long since mentioned that the blood is buffy in some circumstances of that distemper); of Parry; of Geromini; of Abercrombie; of Crampton; of Ayre, and of others. The very curious and interesting observations of Blackall on the connection between sthenic or inflammatory dropsy, and the presence of albumen in the urine, followed up as they have been by those of Drs Bright, James Gregory, and Christison, shewing the dependence of the albuminous condition of the urine on a particular degeneration of the kidneys, and the great liability of dropsy to supervene when this degeneration exists, must be considered as among the most valuable contributions which have been made to medical science in recent times.

The other morbid conditions of the solids and of the fluids which Dr Cullen recognised in his Table and discussed in his First Lines, as capable of occasioning dropsical effusion, though they have been questioned by some, still find their respective advocates among those who have devoted particular attention to this department of pathology, with the exception, indeed, of rupture of lymphatic vessels, and of vesicles and sacs, which are now very generally abandoned.

As a particular illustration of dropsy probably depending on an interruption of absorption, Dr Cullen refers to that so liable to occur in the brain. "As no lymphatic vessels," says he, "have yet very certainly been discovered in that organ, it may be thought that the absorp-

tion, which certainly takes place there, is performed by the extremities of the veins, or by vessels that carry the fluid directly into the veins ; so that any impediment to the free motion of the blood in the veins of the brain, may interrupt the absorption there, and occasion that accumulation of serous fluid which so frequently occurs from a congestion of the blood in these veins. But I give all this as matter of conjecture only." The recent researches of Dr Tonnellé would seem to shew that obliteration of the venous sinuses of the Dura Mater is not an unfrequent attendant of serous effusion into the cavities of the brain ; and it may be doubted whether this effusion should not be attributed to the obstruction, occasioned by the obliteration, to the return of the blood to the heart, rather than to diminished absorption.

Respecting the proximate cause of Scurvy, Dr Cullen says: " Notwithstanding what has been asserted by some eminent persons, I trust to the concurring testimony of the most part of the authors upon the subject, that, in scurvy, the fluids suffer a considerable change." Those who have attempted to explain the nature of the change in the blood which gives rise to the phenomena of scurvy, have strangely omitted to take into consideration Dr Lind's very explicit statement, in the postscript to the third edition of his Treatise on that disease, respecting the very opposite condition in which he had very frequently found the blood of persons labouring under scurvy, from that dissolved, and apparently putrescent, state, which was described by the surgeons of Lord Anson's fleet, and on which he had himself, with most authors, founded his reasoning on the theory of this disease. Though the opportunities of making observations on this and other points con-

nected with the history of sea scurvy have happily been greatly contracted since Dr Lind's time, a strong confirmation of the accuracy of his statement, that the blood of scorbutics was often in a natural state, firm and compact, or even more or less sizey, has been obtained from the analogy of another form of hæmorrhagic disease, the Morbus maculosus of Werlhoff, the Purpura hæmorrhagica of more recent authors, respecting which it has been satisfactorily established by Bateman, Harty, Parry, and others, that in many cases not only does the blood exhibit decided inflammatory characters, but that, in those cases, the treatment is most advantageously conducted by decided antiphlogistic measures. Some recent observations respecting the occurrence of land scurvy at the Cape of Good Hope, seem to shew that the antiphlogistic mode of practice is not inapplicable in the severer forms of purpurous or scorbutic affections.

About the period of the publication of the first volume of the First Lines, Dr Cullen's private practice, in town and country, and both by personal and written consultation, seems to have been considerably extended. His consultation-letters, with his answers to them, which from 1764 he regularly preserved, amounted, in each of the two years 1774 and 1775, to about a hundred; in the following year, they were doubled; and during the last fifteen years of his life, he appears to have carefully considered and answered, on an average, not fewer than two hundred consultation letters yearly.

Nor was the recognition of his rising reputation

confined to his own city and country. On the formation of the Royal Society of Medicine of Paris in 1776, seventeen Foreign Associates were elected. Dr Cullen's name stands fifth on the list, according to the order of their reception, following those of Franklin, Sir John Pringle, Gaubius, and Sir Clifton Winttingham. It was probably the institution of this society which suggested the proposal for a new Academy of Medicine and Surgery in Edinburgh ; a manuscript prospectus of which, bearing date October 1777, I find among Dr Cullen's papers. His nomination as a foreign associate of the Royal Society of Medicine was soon followed by his election as a fellow of the Royal Society of London. His friend Dr William Hunter, in a note dated 16th May 1777, says,—“ Since Thursday se'nnight I have been so much engaged, that I could not well, till now, give you joy of your very honourable election into the Royal Society. It was a very full meeting.” Dr Cullen seems, soon after this, to have taken a zealous and active part in the measures necessary for procuring a charter of incorporation for the Philosophical Society of Edinburgh. I find the following note addressed to him on this subject by his able and distinguished colleague Professor John Robison :—

“ DEAR SIR,

“ *Edinburgh, 6th May 1778.*

“ The constant hurry that I have been in for some time past in order to finish my College, has prevented me from giving that attention to your Memorial that I ought to have done. Inclosed you have a scheme of resolutions, formed agreeably to our sentiments when we met at your house. I have omitted those passages only which I considered as

either prefatory or as not required to recommend the points which you had at heart ; and I have drawn it up in form of resolutions to be made by the Society, as I took that to be the design of committing the paper. I have also put it into the form of a report, and for this reason have signed it, but, I fear, inadvertently, not having recollected that the Chairman alone of the committee should have done this ; but this blunder is venial.

“ I am obliged to go to the country, and have therefore troubled you with this packet ; and hope, on my return next week, to hear that your laudable efforts have been successful. I am, dear Sir, your obedient humble servant,

“ JOHN ROBISON.”

The following letter, addressed by Dr Cullen to Mr Dundas, then Lord Advocate of Scotland, in 1782, relates to the same business :—

“ MY LORD,

“ EDINBURGH, 14th December 1782.

“ As Lord Kames, President of the Philosophical Society of Edinburgh, is at present so much indisposed as to render it inconvenient for him to attend their meetings, I, as Vice-President, have the honour of transmitting to your Lordship the Memorial herewith inclosed. In doing this, I beg leave to observe, that the Philosophical Society have many reasons for desiring to be formed into a body corporate, and particularly for the purpose of legally holding property, in which, for want of a proper constitution, they have formerly suffered a considerable loss. In this view, the Society might apply for their own purposes alone ; but your Lordship will perceive from their memorial, that they are willing and desirous to be comprehended in an institution which, they think, will be of more general utility and credit to this country. This they hope your Lordship will patronize. \* \* \*

“ I have the honour to be, with the utmost respect, my Lord, your Lordship's most obedient and most humble servant,

“ WILLIAM CULLEN.”

As the reward of his exertions, Dr Cullen had the satisfaction, in 1783, of seeing the Philosophical Society erected into the Royal Society; an institution which has since continued to cultivate the various branches of physical science with much industry and success.

In 1778, Dr Cullen effected the purchase of a small landed property named Ormiston Hill, situate in the parish of Kirknewton, about eight miles west from Edinburgh. A passage of a letter formerly given (vol. i. p. 565), addressed to his friend Dr Hunter, contains a simple and pleasing portraiture of his feelings in regard to this acquisition, which continued, during the remaining part of his life, to form an object of great interest to him, as well as an occasion of agreeable relaxation. The following letter, addressed by him, four years afterwards, to Dr Betham, a very early and intimate friend, presents a no less lively and agreeable picture of the pleasure he enjoyed in the cultivation of his farm.

Dr CULLEN to Dr BETHAM, Isle of Man.

"MY DEAR FRIEND,

EDINBURGH, 2d October 1782.

"I regret exceedingly that we have so little intercourse. That I foresaw long ago; but I have often comforted myself that our families should know that their fathers had been friends, and consider themselves as related. This I hope has happened; and, by very slight efforts on our side, I have experienced the warmest expressions of friendship from your family. The attention and kindness which my daughter Margaret\* has, for so long a time, met with from you and your family, must engage me and all mine to you

\* Authoress of the novels, "Home" and "Mornton."

for ever. I find my girl, who is always sincere and honest, feels it with affection and gratitude; and I cannot let her leave you without telling you both her feelings and those of all my family.

“I don’t know how you mean to dispose of your son; but I have had some hint of your thinking of the medical line for him, and I shall be happy in it, as it may give me a means of shewing my regard for both the father and son. As the son shews as good parts as I know the father to have, I think I can be extremely useful to the former, and the giving satisfaction to the latter will be a constant engagement to my attention.

“But let me quit this serious subject, and indulge, as we have often done, in a little badinage, which, *septuagenario proximus*, I still relish a little. I must tell you that I have got upon my hobby; my amusement is a little farm and a little pleasure-ground. If your daughters had not seen the shabby figure of them, I should have made a fine picture to you; but I beg you will not mind what they say. They have no feeling for the beauty of a field that was a heath, now brought to a rich pasture. They were not so long with me as to have the pleasure that I have in seeing a tree grow ten feet in three years. I have done a great deal, but it is all levelling work; other people cannot know what earth has been moved, but I have had some amusement in the turning of every shovelful. I hope to go on for some years yet; but my greatest pleasure is to get a visit from an old friend, whom I fancy I can interest a little in my improvements, but whom I certainly tire in his attending to my detail of them. I had a visit this summer from my good friend Alexander Wilson.\* I was very intent on pointing out to him the singular growth of my balsam poplars; but when I turned about to see his look of complacent satisfaction, he told me gravely he believed the barometer did not stand so

\* Professor of Astronomy at Glasgow.



phenomena and conditions of the nervous system, which, in stating them to his students, he was accustomed to represent as approximations merely to a theory of medicine more complete than the state of knowledge at the time enabled him to attain. His inquisitive and sceptical mode of teaching physic was calculated, by the doubts which it suggested respecting many received opinions, and by the topics of investigation which it pointed out, to excite a spirit of inquiry in the minds of his students, while it tended to engender a belief among them, that a system of physic, more perfect than that which he had proposed, could only be the result of similar reiterated attempts to generalize the healthy and morbid phenomena of the animal economy. Indeed, the students of the Edinburgh School of Medicine had long been encouraged by their teachers, and by none more than by Dr Cullen, to subject all medical opinions to strict critical analysis and to free discussion, in the societies established for their mutual improvement.

It was in this state of the Medical School of Edinburgh, that Dr Brown ushered into public notice a system of medicine professing to be entirely new in its origin, and demonstratively certain in its principles. The novelty of the terms in which the leading ideas of this system were expressed—the simplicity of the views of health and disease which it professed to exhibit, and the ease with which a knowledge of these could be attained—the very agreeable and rapid method in which cures were, upon its principles, to be effected—the reputation for learning and genius which the author of the *Elementa Medicinæ* had acquired as a private

teacher of medicine—the acknowledged elegance, and, at the same time, the profound, perhaps studied obscurity of his Latin compositions—the length of time it was well known he had been employed in studying, explaining, and zealously defending the doctrines of Dr Cullen—the great and apparently sudden change of his medical opinions, and the boldness of the attempt to injure the character and subvert the authority of a teacher who was now generally regarded as at the head of the medical profession in Great Britain—seem all to have contributed to give a lively interest to the writings and lectures of Dr Brown, on their first promulgation, and to procure for his new system of physio a most uncommon degree of attention from medical men in various parts of the civilized world.

It will readily be admitted by those who are acquainted with the writings of Dr Brown, that Dr Cullen could not, consistently with the respect due to himself and to the station which he held in society, make any reply to the unmeasured abuse with which his medical reputation and labours had been attacked in these writings; but that an attempt should not have been made by some one of those in this country who had studied under him, to call in question the soundness and the originality of many of the theoretical doctrines contained in the *Elementa Medicinæ*, and the pretensions of its author to be a reformer of practical medicine, must be attributed, it is conceived, in part, to the obscure character of many of the topics that were brought under discussion; and in part, also, to the small degree of attention to which those who had a personal knowledge of Dr Brown must have considered his opi-

nions on all matters of medical practice, to be entitled. This silence on the part of those who had studied under Dr Cullen, and who had adopted his opinions, may, in some measure, account for the readiness with which Dr Brown's claims to originality and to the improvement of medical science, so confidently urged by himself and his followers, were admitted by many of the Continental physicians; and for those mistakes with regard to the nature and amount of the services that Dr Cullen and Dr Brown have respectively rendered to the science of medicine, which we find so often repeated in their writings, and—what is still more surprising—in the writings even of several medical men of this country. Now that more than half a century has elapsed since the first promulgation of Dr Brown's system, and that the personal feelings and animosities which the discussion of it engendered, have subsided, a comparison of its leading doctrines with those that had been previously taught by Dr Cullen in his lectures and in his writings, may afford to those who take an interest in the history of medical opinions, some grounds for judging of the extent and validity of Dr Brown's claims to originality and to improvements in the physiological, pathological, and therapeutical departments of medical science.

In stating, with a view to this comparison, in distinct propositions, the fundamental propositions of what has usually been termed the Brunonian Theory of Medicine, I shall express them,—as nearly as I shall be able, consistently with brevity,—in the words employed by Dr Brown himself in his *Elementa Medicinæ*, and in his *Outlines of the New Doctrine*. And in the re-

marks which I propose to offer on these propositions, I shall confine myself to such observations as might have been made at the time of their publication, by any of Dr Cullen's pupils who had enjoyed opportunities of becoming acquainted with his lectures and writings, and with his modes of inquiry and reasoning on medical subjects.

I. "There is in man, and in other animals, a property by virtue of which the application of certain external powers, and the exercise of certain internal functions, affect them, so as to produce the phenomena peculiar to their living state. The external powers are heat, air, aliment, and other matters taken into the stomach, the blood, and the fluids secreted from it. The internal functions are muscular contraction, the exercise of sense, and the energy of the brain in thinking, in passion, and in emotion. The property which distinguishes the living from the dead state of organized beings is named *Excitability*; the external powers and internal functions which bring this property into action are called the *Exciting Powers* or *Stimuli*; and the common effect of their action, which comprehends all the functions of living systems, is denominated *Excitement*." (Elements, § 10, 11, 12, 14, 16; Outlines, § 1, 2, 3, 5.)

From this proposition, it is obvious that Dr Brown has attempted to found the whole of medical science upon the basis of a single and universal physiological principle,—*Excitability*,—from which all the phenomena of life, of health, and of disease, proceed. This principle of *Excitability* was supposed by Dr Brown

to afford as complete and satisfactory an explanation of all the phenomena occurring in living systems, as the principle of gravitation is now very generally allowed, by philosophers, to afford of the motions which take place in the planetary system. Indeed, Dr Brown frequently compares his own discoveries with those of Sir Isaac Newton ; and though he seems sometimes to be doubtful which required the greater effort of genius for their production, he never for a moment hesitates to claim for himself the superiority with regard to the usefulness of his discoveries to mankind.\* So confidently, also, has Dr Brown urged his claims to originality in the discovery of the principle of Excitability, that they have been admitted even by many of those who have been disposed to question the justness of the applications he has made of it to the explanation of the phenomena of health and disease.

The view that has already been taken (vol. i. p. 162) of the opinions entertained by physiologists, in successive ages, with regard to the powers which give rise to the phenomena that distinguish living from dead bodies, seems to render it almost unnecessary to remark, that Dr Brown was not the first who attributed the phenomena of the animal economy to the agency of a single unknown principle, or who proposed to apply to this principle a distinct appellation. The term Excitability, as employed by Dr Brown, and for which he himself, in different passages of his writings,

\* See Outlines, § 137. "When a man," Dr Beddoes shrewdly remarks, "asserts the superior utility of his discoveries to those of Newton, he will with difficulty avoid the appearance of asserting the superiority of his talents." Biograph. pref. to edition of Brown's Elements, p. lxxxvi.

substitutes the term Principle of Life, and that of Life itself, obviously comprehends nothing more than what had been expressed by preceding physiologists and pathologists under the various appellations of Soul, Sentient Faculty, Archæus, Vitality or Vital Principle, and Animal Power, or Energy of the Brain.

That Dr Brown had been anticipated by Dr Cullen in the use of the terms Excitability, Exciting Powers, and Excitement, and in annexing to these terms the leading ideas which he employs them to express, has, it is conceived, been sufficiently established by the numerous quotations that have been made from Dr Cullen's lectures and writings, in the view which has been given of his doctrine of Excitement and Collapse. (See vol. i. p. 311.) The employment of these terms by Dr Cullen could not be unknown to Dr Brown, who for a series of years had been permitted, as his biographers inform us, to read and comment upon Dr Cullen's lectures to his private pupils.

It has, indeed, been repeatedly asserted, that Dr Cullen's idea of excitement has nothing in common with that of Dr Brown. "It is believed," Dr Beddoes says, "that hints thrown out by Cullen, were the seed from which Brown raised his doctrines. The connection between the two men is doubtless favourable to the supposition of a communication of ideas, and it is confirmed by the circumstantial evidence of a common term, from which the other terms employed by Brown might be constructed by analogy." But after alluding to the 130th and some of the subsequent paragraphs of Dr Cullen's Institutions of Medicine, as containing the hints referred to, Dr Beddoes proceeds; "Dr Cullen's idea of Excitement, therefore, has nothing in common with that of

Brown : and, on comparison, I am persuaded it will appear that Brown was very little indebted to the physiology of his master." P. cxlvi. Of the erroneousess of this representation, a perusal of the passages referred to cannot fail, I conceive, to convince the impartial inquirer. Dr Cullen's opinion has been nowhere more shortly stated than in the following paragraph. "We suppose (animal) life," says he, "so far as it is corporeal, to consist in the excitement of the nervous system, and especially of the brain." (Physiology, § 136.) A very different, and, as I believe, a much juster, view of the relation which subsists between the doctrines of Excitement taught by Drs Cullen and Brown, from that adopted by Dr Beddoes, was taken in an elaborate treatise entitled *Strictures on the Elementa Medicinæ* of Dr Brown, by Phinehas Hedges, published at Goshen (in the United States) in 1795. "Unwilling," says Mr Hedges, in speaking of Dr Brown's pretensions to originality, "that the world should believe his system resembled the system of Cullen, he took uncommon pains to amuse the public with the pretended novelty of its principles. It requires but a superficial acquaintance with the works of Cullen to discover that the principle laid hold of by Dr Brown, and carried through his work, is nothing more nor less than the principle of Cullen, used in explaining the different conditions of the brain in Mania and Melancholia, and that principle distorted and mutilated, by his transforming power, into what he confidently asserts to be a new principle in Medicine, and sufficient to explain all the phenomena of health and disease." In one important respect, indeed, the notions of these authors differ. Dr Cullen, in recognising the great influence which the nervous system exercises over all the dif-

ferent functions of the animal economy, and in designating this power or influence, on which he conceived the maintenance of life essentially to depend, by the name of Animal Power or Energy of the Brain, and its more or less active states by the terms Excitement and Collapse, admitted the existence in the animal economy of other distinct powers, particularly Irritability and Sensibility,—powers subordinate, perhaps, to nervous energy, but still essential to the exercise of the animal, vital, and natural functions. Dr Brown, on the other hand, represented his Excitability as the sole vital power of the animal economy, holding it to be the one and only internal condition necessary for the production of all the varied phenomena which that economy exhibits.

II. “Excitement, or Life, is not a natural, but a forced state, the tendency of animals being every moment to dissolution, from which they are kept, not by any powers in themselves, but only by the operation of external powers.” (Elements, § 72; Outlines, § 64.)

The opinion that Life is a forced state, constantly requiring the application of external agents for its support, was represented by Dr Brown as peculiarly his own, and is one of those doctrines for the originality of which several of his commentators seem inclined to give him the greatest share of credit, and for which also they consider physiology as most indebted to him. It can scarcely be doubted, however, that Dr Brown must have had repeated opportunities of hearing this opinion expressed by Dr Cullen, as it is to be found in the course of lectures on *Materia Medica* delivered by him in 1761, which Dr Brown attended, and in suc-



cessive courses of his lectures on the Institutions of Medicine and on the Practice of Physic.

“That the soul is constantly necessary to the motions of the body,” Dr Cullen remarked, in his lectures on *Materia Medica* (4to edit. 1773, p. 4), “we readily admit ; but the argument is pushed too far, when it is supposed that these motions are supported by the power of the soul alone ; for it appears that motions, excited by the impulse of external bodies, are absolutely necessary to that support. Take away all impressions on the external senses, and most persons will fall asleep ; and that sleep would probably continue, and end in death, were it not for impressions being renewed.” The late Dr Rush of Philadelphia, in his *Inquiry into the Cause of Animal Life*,\* observes, that “animal life is truly, to use the words of Dr Brown, ‘a forced state.’ I have said the *words* of Dr Brown ; for the opinion was delivered by Dr Cullen in the University of Edinburgh, in the year 1766” [the first year of his lecturing, in this place, on the *Theory of Medicine*], “and was detailed by me in this school many years before the name of Dr Brown was known as a teacher of medicine. It is true, Dr Cullen afterwards deserted it, but it is equally true I never did ; and the belief of it has been the foundation of many of the principles and modes of practice in medicine which I have since adopted. In a lecture which I delivered in the year 1771, I find the following words, which are taken from a manuscript copy of lectures given by Dr Cullen upon the *Institutes of Medicine*. ‘The human body is not an automaton, or self-moving machine ; but is kept alive and in motion by the constant action of stimuli upon it.’”

I have not been able to discover what grounds Dr Rush had for supposing that Dr Cullen ever abandoned the opinion that life is a forced state ; or, in other words, that to maintain the economy in a living condi-

\* *Medical Inquiries and Observations.*

tion, it must be acted upon by certain external agents. For, so far from this having been the case, I find the opinion distinctly maintained in various parts of his writings subsequently to the period to which Dr Rush refers. In his last course of lectures on the Institutions of Medicine, delivered during the session of 1772-3, Dr Cullen observes,—“ Sleep would appear to be the natural state of the human body, and watching the unnatural one. If I can keep off impressions, if I remove, I say, external impressions, there will be no action, but a state of death; whereas, watching does always depend on external powers putting the system in motion; and though the motions produced do indeed contribute to support the same activity, they are all originally derived from some external influence.”—“ Sensation in every shape is a stimulus to our system in one part or other. It was proper to mention this, as it is by the application of sensation that the activity of the system is supported; for withdraw sensation, and the system falls asleep.”—“ The exercise of sensation,” he remarks, in his treatise on the *Materia Medica*, published in 1789, “ is in general a stimulant power, and is a chief means of supporting the mobility of the living principle in the nervous system.”

The same idea is repeated and extended in a passage taken from a manuscript copy of Dr Cullen's lectures on the Practice of Physic, that is quoted in the former volume of this work, in which he states that “ Our system is not a mere automaton, nor supported in its duration by any powers, whether of mind or body, subsisting within itself; it appearing that we have constant need of some external assistance, of external impressions, and if these could be removed, we would not only certainly fall asleep, but we would very soon become dead;” and adds that “ the energy of the brain, which is properly called the Vital Function, depends upon certain other exercises and

functions of the animal economy ; and that both together certainly depend upon the power of external impressions.” —“ External impressions, impulses made upon our external organs of sense, if not the beginning of all motion, are at least a considerable support of it, and are the occasional cause of most of the changes that occur in the motions of the system.” In these passages, it may be observed, it is easy to recognise the source not only of Dr Brown’s representations of Life as a forced state, but also of his distinction of the circumstances on which its maintenance depends, as they consist in the application of certain external powers, and in the exercise of certain internal functions ; or of his doctrine as elsewhere expressed, that “ when the functions of the system itself are considered in their origin, this is found to be partly themselves, partly the external powers.” (Outlines, § 3.)

But in tracing to Dr Cullen’s lectures the more immediate source of Dr Brown’s opinion respecting the dependence of the exercise of the functions of the living economy upon external causes and upon one another, it is not meant to allege that this is an idea which originated with Dr Cullen ; nor does he anywhere lay claim to it as his own. In truth, these opinions are neither more nor less than the ancient doctrine respecting the Naturals and the Non-naturals dressed up in a new guise. “ The true meaning of these appellations,” says Stahl, (*Physiologia, sectio secunda ; De Rebus non-naturalibus*), “ is founded in this, that some things are considered as appertaining absolutely to the internal constitution and existence of the body, and thus constitute, as it were, or enter into, its nature ; and that others belong essentially to its subsistence, but only by a certain external co-operation (*concursu seu occursu*). By the appella-

tion Non-natural, then, properly ought to be understood nothing else than those things which do not indeed appertain to the internal essence of the body, directly or simply, or which, according to the usual mode of speaking, do not enter into the essence of the body for the constitution of it, but are yet of absolute necessity for preserving it and supporting its duration." And after observing that "nature, in so far as it is directly contradistinguished from life, is rather the constitution of the body considered in its full and, as they say, absolute perfection," Stahl goes on to remark that "Life itself, considered in its act (*actu*), appertains, in a much greater degree and more truly, to the non-natural things ; but to the natural, more simply Vitality or Vivacity ; that is, the aptitude and fit disposition for continuing the preservation of the body." Here it seems obvious, that the term Life is employed in a sense pretty closely corresponding with Dr Brown's Excitement, and that of Vitality in a sense corresponding with his Excitability. By no one, perhaps, has the doctrine of the dependence of life on the causes co-operating with the organization of the body, been more clearly and simply stated than by M. Lecaze, in his *Idée de l'Homme*, and in his *Institutiones Medicæ*, both published in 1755. "If there be any means of forming to ourselves a just idea of the laws of the animal economy," says this author, in the former of the works referred to (p. 5 and 2), "it can only be by observing attentively the primary changes produced in us by the causes without which life could not sustain itself, viz. Air, Aliment, Motion and Repose, Sleep and Watching, the Secretions and Excretions, and, lastly, the Affections of the mind. These causes, have been named by the ancients the six *Non-naturals* ; we shall call them *the causes essential to the duration of life*."

"The infant contained in the womb can be considered

there only as parasitical ; but at the moment of its birth, there take place in it revolutions, produced at first by the action of the air, and afterwards by that of the food, which constitutes a new kind of force, suited to give it an existence of its own. This existence becomes more perfect, and at last acquires its full completion by means of all the causes that contribute to the play of life : those causes, which, by the order of their effects, might be divided into primary and secondary, have been named by the ancients the Non-naturals, and we have called them the causes essential to the duration of life." p. 112-13. " We have need of sensations as essentially as of nourishment, and the air which makes us breathe is not more necessary to us for the support of life, than sensations ; since their effects are the principal determining cause of the action of the head." (P. 414.)

And in the *Institutiones Medicæ*, he remarks,—“ It is a common axiom in physiology, that there is no action without a reaction ; every motion, therefore, whether simple or complex, is produced by the union of powers continually striving against one another, and cherishing themselves by their mutual resistance. This axiom plainly shews, that vitality is so constituted that, continually decaying, it would soon come to an end, unless there were present the auxiliary powers afforded by the use of the six *non-naturals*, for cherishing and maintaining it by a due action, or rather counteraction.” (P. 8, 9.)

III. “ Excitability has its seat in medullary nervous matter and in muscular solid, which, together, should be called the Nervous System. It is not different in different parts of its seat, but is one uniform property over the whole system.” (Elements, § 48 ; Outlines, § 31.)

It is in considering the nervous system as the seat of his excitability, that Dr Brown principally differed from

most of those who had, previously to his time, referred the operations of the human economy to a single principle, under the various designations already mentioned. But to those who are acquainted with the views entertained by Dr Cullen respecting the influence of the nervous system in inducing and regulating the phenomena of life, it is unnecessary to remark how completely Dr Brown's opinions on this point were derived from, or modelled upon, those of his preceptor.

In representing the muscular texture as a portion of the nervous system, Dr Brown has advanced, without examination, an opinion as certain, which Dr Cullen had proposed rather as worthy of consideration than as satisfactorily established. "This fourth part of the nervous system," the latter observes, in speaking of muscular or moving fibres, "is the one which is most liable to be disputed and questioned. That it is to be considered as a part of the nervous system, I give entirely as an hypothesis. There is no muscular fibre that is not connected with nerves; and if I find in the muscular fibres the same properties which distinguish medullary substance every where else; if I find these fibres to be organs of sense like other parts, I am ready to conclude that they are a part of the same whole."—"The muscular fibres are sensible to various impressions, and are otherwise organs of the sensations of consciousness. From this, also, it is presumed, that the muscular fibres consist of the same matter which is the subject of sense in other parts of the nervous system." But whilst Dr Cullen conceived muscular fibres to be sensible, he did not confound this property with their irritability. "The muscular fibre," he remarks, "shews a certain property which appears nowhere else in the medullary fibres of any part of the system; and we presume that this difference arises from a peculiar

organization, distinguishing the muscular from the medullary fibre in every other part of the nervous system. If the muscular fibres are a continuation of the nerves, they must have a peculiar organization by which they are capable of peculiar contractility, which, with respect to its causes and manner of operating, is in common to no other bodies in nature, except the living systems of animals and vegetables. To distinguish these from sentient fibres, they are named moving or muscular fibres, and however it may be, the opinion may be allowed that they are a continuation of the nerves. From the strict connection they have with nerves, from the dependence they have upon the nerves and brain, I may presume that they are a part of the nervous system in point of *function*, if not in point of structure and substance."

Dr Cullen was probably the more easily led into the error of considering the muscular fibres as a portion of the nervous system, from his believing, with Dr Whytt, that stimuli act on these fibres only through the intervention of the nerves. But, in the preceding as well as in other passages of his lectures and writings, he has been at great pains to distinguish between the sentient power of the nervous fibres,—the moving power, or irritability, of the muscular fibres,—and the animal or innervatory power, or energy of the brain ; and he accordingly treated of the peculiar properties and laws of each of these three vital powers, separately, in his lectures on the Institutions of Medicine.\* There is, however, it must

\* Physiology, Chaps. i. and ii. See Works, vol. i. pp. 28 and 63. See also vol. i. of this work, p. 277-8, seq. The author of an able article in the first volume of the Edinburgh Medical and Surgical Journal, has made a singular mistake in affirming that we owe to Dr Brown the first attempt to generalize the laws of sensation and of muscular motion.

be acknowledged, a passage in his Lectures on Pathology, in which Dr Cullen has somewhat unguardedly comprehended these three powers under one common term—the *capability of excitement*, a phrase obviously synonymous with *excitability*. The coincidence of the expression employed in this passage with the opinion maintained by Dr Brown, that excitability is not different in different parts of its seat, but is one uniform undivided property over the whole system, is such as seems to leave little if any doubt as to the source from which Dr Brown's idea of excitability was derived. “The power of excitement,” says Dr Cullen, “which distinguishes the vital solid, also distinguishes the moving or muscular fibre, and that power is the common property of medullary fibre. There is something in this fibre which gives the capability of excitement, and it is in common to every part of it; but the brain, as uniting the whole nervous system, has peculiar functions upon which the rest is dependent.” In adopting the view which is here taken by Dr Cullen, of the capability of excitement or excitability as the common property of medullary fibre, Dr Brown has, in the same indiscriminate spirit of generalization, affirmed (Elements, § 15; Outlines, § 5), that sense, motion, the mental function, and the passions, are the only and constant effect of the exciting Powers acting upon the excitability; which effect being one and the same, it must therefore, he conceives, be granted, that the operation of all these powers is also one and the same. “This mode of reasoning,” he observes (Elements, § 15, note *g*) “which is certainly as just as it is new in medicine, will often occur, and we trust will stand the test of the



most scrupulous scrutiny." Few, it is believed, will feel inclined to dispute the novelty of this mode of reasoning, however much difficulty they may have in admitting the justness of the conclusions to which it is supposed to lead—that sense, motion, the mental function, and the passions, are identically the same effect, and that they are all produced by the operation of powers acting by mechanical impulse.

Though it be unquestionably true that the different textures, systems, and organs, of which the animal economy consists, exert more or less of a reciprocal influence on one another, and may be said all to conspire to one general effect,—that of maintaining the body in a living state; yet the co-existing and successive series of phenomena, of which these organic parts are the seats, cannot be considered as of one common kind, or referred to the agency of one common principle. The mechanism by which the blood is circulated through the different regions of the body, and that by which the situation of the body itself, and the relative position of its different parts, can be altered; the process by which the blood is arterialized in the lungs—that by which different products are formed out of it in the minute secretory vessels—that by which these products are again absorbed, and conveyed out of the system—and that by which the waste of the blood thus occasioned, is repaired by the gradual assimilation of foreign matter;—the agency of the brain in the exercise of the intellectual operations—that of the nerves in conveying the impressions of external objects to the mind, and the commands of the mind to the organs of voluntary motion,—and the involuntary en-

ergetory and sympathetic influence of the nervous system upon the different vital and natural functions,—these, though occurring simultaneously or successively in the same general economy, are all distinct operations, produced by organs specially destined for their performance, and depending partly on physical and chemical properties common to all kinds of matter; partly on vital properties peculiar to animal structures; and partly, also, on the mysterious connexion which subsists between the mind and its corporeal organs.

Nothing, certainly, could be more remote from the proper spirit of philosophical inquiry, than to refer, as Dr Brown has done, a number of phenomena, differing so obviously in their nature, as those that occur in the different functions of the animal economy, and in the various textures, systems, and organs subservient to these, to the agency of one common principle or power—Excitability—at a period when the distinct provinces of the Muscular and Nervous Systems, and their respective influence in the functions of Muscular Contraction, of Sensation, of Volition, and of involuntary Innervation, had been recognised, investigated, and distinctly pointed out by several physiologists and pathologists, and in particular, by Haller, Whytt, Bordeu, and Cullen. Indeed, Dr Brown's reference of all the phenomena of the different systems and organs of the animal economy, to the agency of one common principle, does not seem to be more philosophical in its object than the attempt made by some of the ancients to refer all the phenomena of the universe to the agency of one common element; or, than it would be in modern times, to ascribe all the sensible changes

resulting on the surface of our globe from the action of the masses and molecules of different kinds of matter on one another, to the principle of Gravitation alone, to the exclusion of the co-operating principles of Heat, Light, Electricity, Magnetism, and Chemical Affinity.

IV. “ A certain quantity, or a certain energy of Excitability is assigned to every individual system upon the commencement of its living state. The measure of the energy or quantity of this property is different in different animals, and in the same animal at different times and under different circumstances. The effect of the operation of the exciting powers upon the excitability, is to wear it out. The more weakly that stimuli act upon it, it becomes the more accumulated, abundant, or languid ; and the more powerfully they act, it is the more exhausted or consumed. The incapacity for stimulus may go so far as that the smallest portion of stimulus will put an end to life.” (Elements, § 18, 24. Outlines, § 12, 15, 16.)

It is difficult to understand what the precise idea was which Dr Brown wished to express, by affirming that a certain quantity or energy of excitability is assigned to every being on the commencement of its living state : whether he meant to affirm, as his words seem to import, that every organized being receives its whole stock, or sum total, as he himself expresses it (Elem. § 70), of excitability at the commencement of life ; and that this stock, as his account of the effect of the operation of exciting powers necessarily implies, undergoes a gradual but constant diminution from the stimulant operation of all the powers that act upon living systems : —or whether he meant merely to maintain the less ques-

tionable but nugatory proposition that every being, on the commencement of its living state, is endowed with a certain quantity of the living principle, or vital power, which is capable of being increased and diminished by the application and abstraction of stimuli. It is obvious, however, that it is in this last sense only that any definite meaning can be attached to the very frequent use which is made in the Brunonian system, of the terms Accumulation and Exhaustion of Excitability.

Dr Cullen, in proposing the Theory of Excitement, had rejected the terms of accumulation and exhaustion, employed previously to his time in the explanation of the doctrine of the animal spirits, as inapplicable to those conditions of the animal power or energy of the brain, upon which he conceived the states of sleeping and waking, and of vigour and debility, to depend ; conditions which he regarded as consisting simply in the more or less excitable state of the nervous system (see vol. i. p. 310). But Dr Brown, in adopting from Dr Cullen the Theory of Excitement, resumed the use of the expressions of accumulation and exhaustion, and by doing so, has involved himself in some of those contradictions with which he has been charged by the opponents and critics of his doctrines.

In an able article on the writings of Dr Brown, inserted in the first volume of the Edinburgh Medical and Surgical Journal, by my late very intelligent and experienced friend Dr Kellie of Leith, it has been justly remarked, that " Dr Brown has made no provision in his system for the recovery of exhausted excitability, which, indeed, would have been perfectly inconsistent with the fundamental principle of his doctrine, viz. that a determinate portion of excitability is assigned to every in-

dividual at the commencement of its existence. That this determinate portion should be acted upon by stimuli, gradually wasted by their regular operation, or suddenly exhausted by the inordinate action of the more energetic, is intelligible enough, and perfectly congruous to the theory; but that this excitability, the quantum of which is originally so exactly proportioned and so nicely determined for each individual, should, after being consumed, ever again accumulate by any interruption in the application of those stimuli, is neither clear, intelligible, nor consistent. Upon the principle, indeed, thus gratuitously assumed, the excitability might be economized, but never could be increased, never made to accumulate above what remained at the time any proportion of stimulus was withdrawn; though something might be saved, nothing could be gained by the abstraction of stimuli; and on the re-application of the discontinued stimuli, or of others, the excitability should never be found increased or accumulated, but should be stationary at the point of interruption; or more correctly, in every possible case, should be found somewhat wasted, though less than it otherwise might have been, inasmuch as it must have been acted upon, and consequently consumed more or less by the other continued stimuli, in proportion to their number and power.\* On the other hand, every additional, unusual, and violent stimulation should, upon the same principle, rapidly waste and hasten

\* In reference to this opinion of Dr Brown's, Professor Vacca of Pisa has observed,—“ We may form an idea of the logic of this author from the following reasoning: Excitability is of a determinate degree in each individual, and is continually consumed by an external power, but sometimes in larger and sometimes in less quantity. When it is consumed in a lesser degree, instead of diminishing it increases! This seems to be nearly the same as to say, I have a purse containing 200 pieces of money. In general I take out six pieces every day, but some days I take two only; and whenever I take only two, the number of the pieces of money is increased! *Risum teneatis amici.*”

the irrecoverable exhaustion of the powers of life. The proposition, then, that a determinate portion of excitability is assigned to every individual, being equally inconsistent with the subsequent theorems of the system and with matter of fact, must be given up, or explained away somehow. This the later, or Pseudo-Brunonians, have attempted; or they have yielded the point so far as to admit that the wasted excitability may, by rest and food, be partially renewed, 'though it does not entirely regain the point where the exhaustion had commenced.' This concession, however awkwardly and sparingly made, is, in truth, an acknowledgment of oversight in that brilliant system which challenges all reproof, and by no means a refutation of the critique of Hufeland, who had before pointed out the inconsistency of this principle."

It would be endless to enumerate all the contradictions in which Dr Brown was involved by his theory that life consists in, or depends on, the gradual exhaustion of the stock of excitability which every being receives at the period of its formation. As examples, it may be sufficient to mention his declaration, that the more abundant the excitability is, the more it is languid (*Outlines*, § 12), the more easily it is saturated, and the less stimulus does it admit (*ibid.* § 14); that the greatest degree of excitement is produced by the action of a stimulus of a medium force on a half-wasted excitability (*Elements*, § 25); that the excitability may be increased by being less wasted (*Elem.* § 24, 32, 39); and that death, which, he says, is a consequence of a perfect extinction of the excitement, may arise either from a complete exhaustion or an ultimate abundance of the excitability.

V. "As some of the Exciting Powers act by evident

impulses, and the identity of the effect of the others infers the same mode of operation, and as they have all a certain activity in them, they ought to be denominated stimulant or stimuli. And since the exciting powers produce all the phenomena of life, and the only operation by which they do so is stimulant, there can consequently be no sedative power in nature, all the phenomena of life, both in health and disease, consisting in, or being produced by, the operation of stimuli." (Elements, § 17, 22 ; Outlines, § 8 and 9.)

That Dr Brown's view of the exciting powers as the agents which produce all the phenomena of life, does not differ from that which had been given by Dr Cullen, of the nature and operation of stimuli, will be rendered apparent by the following extracts taken from his lectures on *Materia Medica*, and on *Therapeutics*: "Whatever," says Dr Cullen, "excites motion in an animal body is a stimulus,—medicines which have this effect are called Stimulants."—"Stimulants are those powers which excite or increase in the human body all the motions that are peculiar to it. They are such powers as act upon the vital solid, and not upon the dead body. At the same time I must own that what has been chiefly taken notice of is their exciting the action of the muscular or moving fibres."\*

In assigning it as a reason for calling the exciting powers Stimuli ; that "some of them act by evident impulses, and that the identity of the effect of the others infers the same mode of operation," Dr Brown has adopted, without any limitation, an opinion which

\* De Gorter states as the general law of stimulants, "*omnes partes mobiles in viventibus a stimulo in majorem motum et constrictionem adigi posse.*" (*Medicinae Compend. Tract. 84. § iv.*)

Dr Cullen seems to have taken incautiously from Mr Locke, that "impulse is the only way in which bodies can be conceived to operate upon us;" an opinion which Mr Locke himself, becoming convinced as he states, "by the judicious Mr Newton's incomparable book, that it is too bold a presumption to limit God's power in this point by my narrow conceptions," promised to have rectified in the next edition of his work. "As we know," says Dr Cullen, "of no other action of bodies on each other but that of impulse, and as in the case of the sensations of the four first genera (viz. sight, hearing, taste, and smell) we learn that an impulse takes place, we have comprehended the whole under the title of sensations of impression, and consider all of them as perceptions of impulse." (Outlines of Physiology, § 41.) Dr Cullen, however, seems to have been aware that the supposition of the phenomena of living systems, being produced by the operation of impulse alone, is founded upon a partial consideration of these phenomena. "I must observe," he remarks in his lectures on Therapeutics, "that, from our ignorance of vital phenomena, the operation of stimulants is not understood. We may fancy that we have some idea of impulse as a stimulant, but we will not apply this to the slightest wound of any kind which proves painful and stimulant." After this acknowledgment on the part of Dr Cullen, it would be superfluous to insist on the imperfections of an hypothesis which pretends to explain all the actions produced in the animal economy by mechanical impulse, to the exclusion of the operation of vital energies as well as of chemical affinities.



If the second reason assigned by Dr Brown for denominating the exciting powers stimulant or stimuli, viz. that they have all a certain activity in them, has any meaning, this can only be that they act, or have the power or quality of acting, or, in other words, that they stimulate. Action, stimulation, and excitement, therefore, according to Dr Brown, must be regarded as synonymous terms, expressive simply of the effects resulting from the impulse, or stimulating operation of external powers upon the animal economy, whether this shall be followed by an increase or a diminution of its vital motions.

With regard to Dr Brown's assertion, that there can be no Sedative power in nature, it may be observed, that three classes of remedies have usually been included by medical writers under the general term Sedative, those which have been supposed to have the effect of diminishing the vital motions, more strictly termed Sedative,—those which allay pain, Anodynes,—and those which induce sleep, Hypnotics. It is obvious, however, that the term Sedative, as opposed to Stimulant, can properly be understood only in the first of these significations, and in this view, Sedatives have been defined by Dr Cullen to be such substances as diminish motions in the system and the force of the moving power. But the assertion of Dr Brown that there exist no such substances as Sedatives in nature,—“all the bodies which seem to be sedative, owing their debility to a degree of stimulus greatly inferior to the proper one” (Elements, § 21),—instead of being, as it is in his writings, a simple inference, or rather assumption, from a vague and erro-

neous definition of Stimulus, would have required for its establishment to have been deduced from a careful and accurate examination of the peculiar effects produced, immediately and more remotely, on the moving powers of the animal economy, by the different substances which are capable of influencing them.

The substances that have generally been supposed to have an immediate effect in diminishing the action of the heart and arteries, and in weakening the action of the respiratory organs,—the effects by which the sedative power of bodies has usually been estimated,—belong almost entirely to that class of agents which have been denominated Noxious Powers, or Poisons. Dr Cullen had stated that Human and Marsh Effluvia seem to be, in their primary operation, of a sedative or debilitating quality, and that the increased action of the heart and arteries which occurs in the fevers they occasion, is not to be regarded as the direct and simple effect of these powers, but as a phenomenon arising from the reaction of the system. In opposition to this opinion, Dr Brown contends, that, as the operation of the hurtful powers producing diseases usually depends upon stimulus, and as the effects of contagions are precisely the same as those of the other hurtful powers, therefore the operation of contagions also must be admitted to be stimulant. (Elements, § 21.) “The great debilitating energy,” he adds, “observable in certain contagions, does no more prove a diversity of action in them, than it does in the case of an equal or greater degree of debility arising from cold.” But in employing this mode of reasoning, Dr Brown seems to have forgotten that cold consists merely in the abstraction of heat

or stimulating power ; whereas, in producing diseases, contagions must operate on the body as powers super-added to those by which its ordinary conditions are maintained; that the debility which they produce is not, to use his own language, “ the effect of the privation or want of something which, when added, would give vigour or excitement ;” but “ the effect of the addition of something endowed with a positive power to diminish or destroy excitement, or the state of living systems.” (Outlines, § 7.) In the present state of our knowledge, however, the numerous instances, in Fevers of all sorts, in Contagious diseases, and in Epidemical distempers, in which death has been observed to occur without any symptom of increased excitement, stimulation, or reaction of the system having ever manifested itself, must, it is conceived, be regarded as so many proofs of the directly sedative or debilitating action of human contagions, and of marsh effluvia.

In investigating the action of animal, vegetable, and mineral poisons upon the animal economy, medical men seem to have paid less attention to their immediate effects upon the Sanguiferous and Respiratory systems, than to the changes which they produce upon the Nervous system, upon the Digestive organs, and upon the different Surfaces of the body to which they are directly applied. But if any reliance may be placed upon the accuracy of experiments made with different poisonous substances upon brute animals, or of observations made upon man in cases of accidental or intentional poisoning, no reasonable doubt can be entertained that many of these substances have the effect of diminishing directly, and without any marks of previous excitement,

the action of the heart and arteries, and of the respiratory organs, and of inducing coldness of the surface of the body, languor, disposition to syncope, and other symptoms of general debility ; in short, of producing all the effects of sedative or debilitating, as contradistinguished from those of stimulant, powers.

The only argument which Dr Brown advances in support of his proposition, that no such substances as Sedatives exist, is a reference to several instances in which a sedative effect is produced by the diminution or abstraction of the ordinary exciting powers. (Elements, § 21, 2, 4, 7, 8.) That a sedative effect may be produced in this manner has been long known to physicians, and forms the basis of what is familiarly known by the name of the Antiphlogistic Regimen. But this obviously affords no proof of the non-existence of a class of substances possessing actually sedative or positively debilitating powers ; an inference which would require to be founded on an examination of the effects of all the different substances that are capable, when applied to, or introduced into, the system, of affecting the propelling power of the heart and arteries.

VI. "The excitement of living systems varies according to the degree of excitability which has been assigned to each, and according, also, to the number and degrees of the stimuli which have been applied. Health and disease are the same state, depending on the same cause, that is Excitement, varying only in degree." (Elements, §23, 65 ; Outlines, § 10, 13, 53, 55.)

That the effect of a stimulus must be in a ratio com-

pounded of its force and of the more or less excitable state of the part to which it is applied, is a proposition which is not likely to be contested. It was stated generally by Dr Cullen in the following terms :—

“ The effect of every machine will be as the power applied and the nature of the machine, taken together.”—And again, more particularly : “ Every action, every motion, that occurs in the human body, may have a comprehended cause, and the exact state of it is almost always as the stimulus applied, and the mobility of the system, taken together. Increased motion, therefore, may be considered as arising either from the power of stimuli that are unusual, or from the usual stimuli being increased ; or, the stimulus being given, it may arise from increased tone and vigour, or from increased mobility.”

In affirming that health and disease are the same state, depending on the same cause,—that is, excitement,—varying only in degree, it is obvious that Dr Brown merely expressed that they are both living states ; that where there is no life, there can be no disease, any more than there can be health,—a proposition of undeniable truth, but which certainly does not add much to the stock of our medical knowledge.

VII. “ Predisposition to disease is a middle state of Excitement between perfect health and disease, produced by the same exciting powers that give rise to these two states, and always precedes universal, but never local, diseases.” (Elements, § 6 ; Outlines, § 65, 52.)

In regarding Predisposition as the middle state of Excitement between perfect health and disease, and in assigning to that state a certain range on each side of

perfect health, Dr Brown seems merely to have adopted the common opinion of physicians, first very accurately expressed by Galen, and of which Dr Cullen was accustomed to give the following account in his lectures on Pathology :—" We can imagine a standard of health ; but that perfect standard exists nowhere in any one person. Physicians have been long sensible of this, and therefore they have invented a term, Latitude of health. They suppose that health may deviate, on either side, from the standard without passing to the opposite state, that of disease. If I were to attempt to establish a standard of health, it would consist in a certain conformation of the body, in a certain vigour of actions affixed to a certain time of life, &c. ; and young persons not arrived at that period, or old persons who have gone from it, though they cannot rightly perform all their actions, are still considered as not under disease."

That predisposition may be produced by the same causes that give rise to the states of health and disease, is another opinion which certainly did not originate with Dr Brown, but one which has been maintained by pathologists in every age, in relation to the operation both of the common supporters of life, and of several of the ordinary exciting causes of diseases. Abundant proofs of this are to be found in every medical work which treats of the exciting and predisposing causes of diseases.

It is not easy to conceive by what train of observation or reasoning Dr Brown was led to conclude that Universal diseases are always, and Local diseases never, preceded by Predisposition ; for both parts of the affirmation seem to be equally erroneous. That Universal diseases may arise without any peculiar liability to their attacks having been previously engen-

dered *by the operation of their exciting causes*, we have every day proofs, in the production of intermittent and of contagious fevers of all sorts, from temporary and occasional exposure to the operation of their respective causes. And, on the other hand, there can be no question that many individuals are more disposed than others to particular local diseases, such as scrofulous and cancerous tumours and ulcers ; or that the same individual may be more disposed to such diseases in one period of his life than in another.

VIII. "Debility may be the consequence either of the excess or of the deficiency of stimuli ; the debility which is occasioned by an excess of stimuli is termed Indirect ; and that which arises from a deficiency of stimuli, is denominated Direct debility. Death, therefore, may occur either as the consequence of an excess of stimulant power exhausting the excitability irreparably, or in consequence of a diminution of the excitement from a deficiency of stimuli, and the accumulation of Excitability." (Elements, § 35 and 45. Outlines, § 22 and 29 ; 18 and 26.)

The distinction made by Dr Brown between the debility arising from a deficiency of stimuli, which he denominates Direct, and that which is induced by the excessive use of stimuli, termed by him Indirect Debility, has by many been regarded as the most original part of his Theory of Medicine. That Dr Cullen, however, had recognized and distinctly pointed out the two kinds of causes from which debility may proceed, is evident from the account formerly given (vol. i. p. 371.) of the various circumstances which he

enumerated as capable of producing that state; on the one hand, exercise carried to fatigue, excessive heat, every unusual degree of excitement, and repeated excitements, at length wearing out the system—the causes of the state which Dr Brown denominated indirect debility: and on the other hand, indolence, cold, the withdrawing of blood from the vessels of the brain, want of nourishment, copious evacuations, and the absence of habitual stimulants,—the sources, obviously, of Dr Brown's direct debility.

With regard to the debility arising from excess of stimulant powers, denominated Indirect by Dr Brown, Dr Cullen had remarked in his lectures on Pathology, that “such is the constitution of the nervous system, that every unusual degree of excitement is followed with a proportional degree of collapse.”—“Any power,” he remarks, in his lectures on Therapeutics, “exciting the nervous system in a very considerable degree, is followed by a proportional degree of collapse; and when the excitement is to a certain degree, the collapse may be fatal.”—“The excess of stimulant power,” he observes in another place (Works, i. 563), “may be dangerous, merely by the repeated and violent excitement, and the state of debility that necessarily follows.”

With regard, again, to the debility produced by the abstraction of stimuli—denominated Direct by Dr Brown, Dr Cullen, in his lectures on Pathology, has made the following observation. “Another occasional cause of debility is the withdrawing of blood from the vessels of the brain; and as the vigour of the brain depends upon the state of tension in the whole of the system, debility



may be produced by the want of nourishment, by evacuations, and by the absence of habitual stimulants. Where stimulants are daily applied, as in the case of wine, the system comes to depend upon them for its degree of tension, and, therefore, the absence of these has a remarkable effect in inducing debility. Thus, in the case of persons who use the habitual stimulus of a dram, when that stimulus should have been renewed, what a sense of debility, what tremor occurs, till the tension is reproduced by the application of the dram to the stomach." From these quotations, it is obvious that the facts relative to the two kinds of debility, so much insisted on by Dr Brown, had been generalized by Dr Cullen, and there remains for Dr Brown only the merit of having applied the term *direct* to the state of debility which is produced by a deficiency of the natural stimuli, and that of *indirect* to the form of debility which is occasioned by an excess of stimuli:—terms which, however convenient they may be in pathology, for the sake of the brevity and variation of expression which they afford, are far from being happily chosen, inasmuch as the connection between them and the states they are employed to designate, is so loose and arbitrary, that they might, without any verbal impropriety, have been exchanged or substituted for one another.

It may be mentioned, also, that the words Direct and Indirect are terms which had been previously applied by Dr Cullen, to two different kinds of causes by which the motions that are peculiar to the human body are excited in it. Those agents which have a direct and manifest power of increasing motion in the system, he included under the general denomination of Direct Stimulants; whilst to certain other powers which,

though they have no such direct and manifest power of increasing motion in the system, do still, in consequence of a law of the animal economy, occasion an exertion of the animal power, he gave the name of Indirect Stimulants. "Nothing," he observed, "can shew better that active powers can be excited by a sense of debility, than this, that if a stimulus accustomed to support the activity of the system happen to be withdrawn, the sense of debility thence arising produces various actions in the system, or in particular parts of it. All these means of exciting the action of the system, or of particular parts, we name Indirect Stimulants." It seems obvious, therefore, that the state which Dr Brown has termed indirect debility, arises from an excessive use of those external agents which Dr Cullen denominated direct stimulants, and that it is the state of what Dr Brown called direct debility that is the cause of those motions which Dr Cullen attributed to the operation of indirect stimulants.

IX. "Stimuli which produce a degree of excitement greater than that of health, give rise to diseases that are denominated Sthenic, or diseases of excessive vigour, or of a high luxuriance of health ; while those that produce a degree of excitement less than that of health, give rise to diseases which may be denominated Asthenic, or diseases of Debility. Sthenic and Asthenic are the only two forms of Universal diseases, as well as of predispositions leading to, or of diatheses existing in, each of these forms. (Elements, § 66, 68 ; Outlines, § 10, 56, 75.) Asthenic diseases, including those of both direct and indirect debility, occur much more frequently than sthenic diseases, the proportion

of the former being to that of the latter not less than ninety-seven to three in the hundred." (Elements, Notes on § 98, 197, and 493 ; Outlines, § 83, 107 ; Observations on Spasm, Note on § 34 ; &c. &c.)

It is impossible to read Dr Brown's account of health, as holding the middle place between the effects of excessive and deficient stimulus in which disease consists, without recalling to recollection a passage already quoted from Dr Cullen's lectures on Physiology, in which he assumes the waking state of a man in health as the point from which to estimate the different degrees of augmentation and decrease of which the excitement of the nervous system is capable. "I have been," he observes, "a little at a loss in the application of the terms of Excitement and Collapse. There is nothing more manifest than that the degree of Excitement is very different upon different occasions ; if we take the lowest degree of vital existence as the point from which to reckon, every higher degree than that must be called a degree of Excitement ; and if, again, we take the highest degrees, and consider the lower degrees that may take place while life still subsists, every lower may be called a degree of Collapse."

It has been already remarked how imperfect a view of the animal economy *in the state of health*, could be obtained from regarding the functions of the different textures, systems, and organs of which it is composed, as being all of one common kind, and produced by the agency of one common property. And it is obvious that the extension of the same mode of reasoning to the explanation of the morbid phenomena that are exhibited by the different parts employed in the per-

formance of the Circulatory, Nervous, Secretory, Nutritive, and Reproductive Functions, must give rise to an equally limited and unsatisfactory view of the animal economy *in the state of disease*. The legitimate object of pathological inquiry seems to be, to endeavour to ascertain to what particular states, structural and dynamical, of the organs concerned in the performance of the different vital, animal, and natural functions, their various derangements should be referred ;—or, in other words, to ascertain analytically, as far as may be possible, those simple, distinct, and evident deviations from healthy action—the *morbi simplices* of pathologists, or proximate causes of Dr Cullen—which, by the various combinations of symptoms to which they give rise in concurrence and succession, constitute particular diseases ; instead of confounding, as has been done by Dr Brown, all the morbid conditions of the different organs and functions of the animal economy, under terms so very indiscriminative and general as those of increased and diminished Excitement.

The idea of referring all diseases to two opposite conditions of the animal economy,—the states of Vigour and Debility,—which forms so prominent a part of the theory of Dr Brown, seems to have originated with the ancient Methodics, and to have undergone various changes in the modes of its expression, and various limitations in regard to the extent of its application, according to the pathological theories of the periods through which it has passed. It is in this light, it is conceived, that we must regard the doctrine of Strictness and Laxity of the Pores, to which dis-

eases were ascribed by Asclepiades ; that of Tension and Laxity of the Fibres, advocated by Baglivi ; that of Increased and Diminished Motions, or Spasm and Atony, proposed by Hoffmann ; that of Irritability and Torpor, maintained by Glisson and Gaubius ; and that of Increased and Diminished Excitement or Reaction of the System, to which so much importance was attached by Dr Cullen.

It seems, however, to have been the employment, by Dr Brown, of the Greek terms *Sthenia* and *Asthenia*, in place of those employed by his predecessors to denote the two opposite pathological states of Vigour and Debility, or of increased and diminished action in the system, that has given an appearance of novelty to his division of diseases. The influence of the imposition of new names upon old and familiar ideas, has been well understood in every period of science, and has been had recourse to, not unfrequently, by those who, like Dr Brown, have been ambitious of being regarded as the founders of theories or systems entirely new in Medicine.

Gaubius, in his Pathology, had said, that the " Vital power can be faulty in two ways, viz. in excess and in deficiency. The former," he says, " I call Irritability, the latter Torpor. The middle state, between the two extremes, constitutes health. Irritability," he adds, " as I understand it, is when the sensibility of the living solids is so great, that, upon the slightest irritation, it breaks out into such violent motions as disorder the tenor of the functions. Torpor infers a diminished sensibility of the living solid, by which it happens that the effort to contraction, upon any irritation made, is so small as not to be sufficient for produc-

ing those motions which the health of the economy requires."

Dr Cullen, in commenting on this paragraph, in his lectures on Pathology, has shewn that the generalization of morbid states proposed by Gaubius is, in several respects, imperfect and erroneous ; *first*, from its making no distinction between the force and the facility with which muscular contractions are performed when stimuli are applied to the system ; and, *second*, from its making no distinction between the properties of Sensibility and Irritability, which had been so accurately distinguished from one another by Glisson and by Haller. It will be found that these remarks of Dr Cullen's are equally applicable to the generalization of morbid states proposed by Dr Brown, which, indeed, differs from that of Gaubius merely in the substitution of the term Excitability for that of Vital Power, and in his incongruous use of the epithet Languor from Accumulation of Excitability, to express that state which Dr Cullen had previously denominated Mobility of the System.

In estimating the degrees of Vigour and Debility as they occur in individual cases of disease, two different standards have usually been had recourse to. The most obvious of these, and that which is commonly appealed to by the vulgar, is the degree of muscular force which can be exerted in the performance of voluntary motions ; the other is the force with which the blood appears to be propelled by the heart and arteries through the different regions of the body. It is by this latter standard, the force of the circulation, that scientific medical practitioners have long been

accustomed to judge of the degrees of morbidly increased and diminished excitement, or, in other words, of the states of Vigour and Debility that prevail in the economy; following in this respect the recommendation of Galen, to judge of the strength of the patient by examining into the state of his pulse.\* The term Excitement, as employed by Dr Brown, is obviously not limited either to the powers of voluntary motion, or to the state of the circulation, but comprehends the exercise of all the functions of the animal economy; and accordingly, when he speaks of the existence of morbidly increased excitement, or sthenic diathesis, we are to understand that the whole of the functions are carried on with an unusual degree of vigour; whilst, on the other hand, in the state of morbidly diminished excitement, or of asthenic diathesis, the whole of the functions are supposed to be carried on very feebly. Daily experience, however, evinces that, in diseases, not only may the different systems and organs of which the animal economy is composed, exist at the same periods of their progress, in very different conditions from one another, in respect of Vigour and Debility, but that even different parts of the same system,—as of the circulatory and nervous, for example, may be, at the very same time, in opposite states of excitement. Although, therefore, the terms Sthenic and Asthe-

\* “*Maxime autem ac proprie,*” says Fernelius, “*virium robur atque constantiam pulsus ostendit. Facultatum, quippe, omnium princeps atque conservatrix est vitalis, qua una constante nemo interire potest, ex qua et vita omnis, et virium robur æstimari solet.*” (*Patholog. lib. iii. cap. 7; Virium Observatio ex Pulsu, vol. i. p. 246.*)

nic may, in such circumstances, be applicable to the state of particular functions, or of particular parts of a function, they cannot, with propriety, be applied to the state of the whole of the functions at once, as has been done by Dr Brown, nor, consequently, be affirmed generally of any particular disease with which the system may be affected.\*

Dr Cullen, who had himself introduced the term Excitement into general medical use, as a term synonymous with the effect of the increased animal power or energy of the brain, had been at particular pains in pointing out the important fact of the frequent co-existence of the opposite states of vigour and debility in the different functions of the animal economy in the account which he gave of the phenomena of Febrile, Inflammatory, and Nervous diseases. "How

\* The very different conditions in which the several functions of the economy may exist simultaneously, is very explicitly stated by Fernellius in the following passage: "*Itaque potest quævis una facultas noxam et incommodum subire, reliquis aut nondum aut non peræque læsis; quin fortasse potest in quapiam corporis particula vis animalis ex morbo perire, simul cum hac et naturalis, una superstite vitali. Unde plane perspicui potest, tres illas non modo cogitatione et (ut dicere solent) ratione, sed re ipsa propriaque substantia disjungi.*" (Physiolog. lib. v. cap. 13, vol. i. p. 117.) And Sennertus very distinctly recognises the independence of the nervous and sanguiferous systems, in respect of strength of action, in what he says *De Virium Imbecillitate* (ii. 770-1); for after observing that "*Virium defectus et imbecillitas dicitur, cum vires vitales atque cordis et arteriarum, præcipue læduntur,*" he contrasts with this the "*motus impotentia, quæ, viribus et actionibus cordis salvis, nervoso genere imprimis affecto, accidit.*"



the different portions of the brain," he remarks, " may at the same time be excited or collapsed in different degrees ; or how the energy of the brain may be in different degrees of force with respect to the several vital, animal, and natural functions, I cannot pretend to explain ; but it is sufficiently evident in fact, that the brain may be, at one and the same time, in different conditions with respect to these functions. Thus, in inflammatory diseases, when, by a stimulus applied to the brain, the force of the vital functions is preternaturally increased, that of the animal is either little changed or considerably diminished. On the contrary, in many cases of mania, the force of the animal functions, depending always on the brain, is prodigiously increased, while the state of the vital function in the heart is very little or not at all changed."—" The phlogistic diathesis," Dr Cullen further observes, " consists in an increased tone of the arterial system, and is certainly partial in relation to the general system ; for commonly there is, at the same time, a diminished tone of the other muscles, particularly of those of voluntary motion, and often of those of the alimentary canal."—" Sensibility and irritability are not always in the same condition in the same person."—" What connection there is," he observes, in speaking of Maniacs, " between the diminution of sensibility and the increase of strength, it may be difficult to explain. It is difficult in theory, and I do not attempt it ; but the combination is frequent." (See also Works, vol. i. p. 589.)

Another circumstance which renders Dr Brown's division of diseases into Sthenic and Asthenic incorrect and unsatisfactory, is the well-known fact that the state of particular functions, affected with morbid degrees of vigour and debility, is not necessarily permanent throughout the whole progress of any particular disease, but that in the successive stages of a disease

these opposite states may follow one another, so that sometimes the one state and sometimes the other may be said to predominate. The examples of this succession, and occasionally sudden alternation, of the states of vigour and debility in the vital, animal, and natural functions, which occur in the progress of febrile, inflammatory, and nervous diseases, are too numerous and too well known to require illustration.

Not only does there occur in diseases a difference in respect of vigour and debility, or of sthenic and asthenic diathesis, in the state of the various functions at the same period of time, or of the same functions in the successive stages of a disease, but each of these functions may be differently affected in these respects in different individuals labouring under the same specific disease. Dr Brown himself seems to have been in some degree sensible of this, and accordingly he has placed several diseases, such as Small-pox and Scarlatina, both in his sthenic and asthenic class of diseases. But if he had extended the same principle of arrangement, derived from the sthenic or asthenic character of diseases, to all those to which it is applicable, the number of diseases requiring to be included in both his classes would have been much greater than he seems to have been at all aware of. Thus, in epistaxis, dysentery, common cholera, croup, synochus, typhus, the plague, and in intermittent and remittent fevers, which are regarded by Dr Brown as diseases of debility, as purely asthenic diseases, every one knows that a high degree of phlogistic or sthenic action not unfrequently prevails, and forms the principal source of danger. And no one who has witnessed

the epidemic prevalence of measles and erysipelas, diseases which are represented by Dr Brown as of a purely sthenic character, can have failed to observe many cases in which the symptoms of debility in the vital, animal, and natural functions were, from the first, so great and so distinctly marked, as to baffle the utmost efforts of art made to overcome them.

It is obvious, therefore, from the variety of appearances which the same specific diseases may exhibit, both in different individuals in the same stage, and in the same individual in the different stages of their progress, that the judgment formed with regard to their sthenic or asthenic character, and with regard to the degree in which either of these two states exists, must be confined to the particular period and circumstances under which they are observed, and cannot be extended to their whole course, and to all the variety of circumstances under which they may occur. In the epidemic prevalence of contagious fevers, or of febrile exanthemata, for example, where an opportunity is afforded of observing the effects which the same specific causes produce on a number of individuals, how infinitely diversified are the morbid appearances that each individual exhibits, both in regard to the degree in which the symptoms of the two opposite states of sthenia and asthenia, or of excitement and debility in the vital, animal, and natural functions, manifest themselves, and in regard to the order in which these symptoms succeed one another! It is no doubt true, that, in the epidemics of particular years, there subsists a general resemblance in the character exhibited by the majority of particular cases, whether that cha-

racter be mild, inflammatory, or what has been termed malignant; but, at the same time, it has been long known to every practitioner of experience, that, of a number of individuals examined at an early stage of any epidemic disease, one may be found with symptoms so mild as to present little room for the interference of art; another with marks of inflammatory action so severe as to require the most active debilitating measures; and a third, with all the symptoms of prostration of strength, requiring the use of the most powerful stimuli. In the further progress of the same cases, the individual whose symptoms were mild at the commencement, may continue to be only very slightly affected, or he may be attacked with symptoms either of inflammatory action or of debility. In those, again, in whom the early symptoms of the disease were of a more violent character, whether of an inflammatory or of a typhoid nature, these alarming symptoms may gradually subside, and the disease assume a mild character; or the same symptoms with which the disease began may continue to predominate throughout its course; or they may be succeeded by symptoms of an equally dangerous, though of an opposite nature.

But though the morbid states of the animal economy, indicated by the terms *sthenic* and *asthenic*, or *vigorous* and *weak*, are undoubtedly most important objects of consideration in Pathology, and in the Practice of Medicine, a very slight acquaintance with diseases is sufficient to shew that many of the phenomena which occur in them, whether they have their origin in the solid parts of the body, such as the muscular, nervous, or cellular textures, or in its fluid

parts, the blood and the various secretions from it, do not admit of being explained simply by the existence of the states of vigour and debility. Who that knows any thing of the phenomena of intermittent fevers, of gout, apoplexy, mania, chorea, epilepsy, hydrophobia, chincough, chlorosis, scurvy, pemphigus, lepra, syphilis, scrofula, calculous diathesis, and diabetes, can persuade himself that the peculiar and characteristic phenomena of these diseases differ from one another in their origin and nature, merely in being higher and lower degrees of excitement in the scale of disease, or simple variations in the vigour and debility of the general system, or of the different organs of which it consists ; or that, by a proper adaptation and use of stimulant powers, these diseases could be mutually converted into one another? Yet this hypothesis, in every respect so absurd and untenable, is the basis upon which Dr Brown's division of all diseases into sthenic and asthenic necessarily rests.

With the exception of a few inflammatory diseases, such as pneumonia, phrenitis, acute rheumatism, &c. and of those varieties of smallpox and scarlet fever which are accompanied with symptoms of an inflammatory diathesis (Elements, § 347), Dr Brown has referred all other universal diseases to the class of asthenic diseases, or diseases of Debility (ibid. § 505). If this arrangement—which derives its sole plausibility from a total neglect of the differences which may, and frequently do, exist in diseases, in the states of the vigour and debility of the animal, vital, and natural functions—were allowed to be just, then Dr Brown's frequently repeated assertion might be admit-

ted, that the proportion in the frequency of the occurrence of asthenic to sthenic diseases, is not less than that of ninety-seven to three in the hundred. But it may be left to every practitioner of experience, to decide whether Dr Brown could possibly have hazarded any opinion more wide of the truth than this, or better calculated to betray his total ignorance of diseases, and of the particular modes of treatment which, in their various forms and stages, they may require.

X. "The whole doctrine of Excitability, of Exciting Powers, and of Excitement, admits of being exhibited in a scale expressive of the Accumulation and Exhaustion of Excitability that take place in the various states of health and disease." (Outlines, § 136.)

Dr Brown has attempted, by means of a scale of numbers, to exhibit, in the most compendious form and with arithmetical precision, the whole of his doctrines with regard to exciting powers, excitability, and excitement, in health, in disease, and in the predisposition to disease. "This scale," he remarks (*loc. cit.*), "may be considered as either a scale of human life from beginning to end, or as a scale of all the deviations from the point of health towards either of the extremes of predisposition and morbid state, till the complete dissolution of the living state in death." Of this scale, accordingly, two forms have been described; one (No. I.) by Dr Brown himself, in the 136th sec. of his Outlines, and another (No. II.) by his pupil, Dr Linch, at the end of the *Elementa*, to which reference is constantly made by Dr Brown in the progress of that work.



In the formation of the first of these scales, it is supposed that man at birth is endowed with a certain quantity of excitability, which may be represented by the number 80; the mark at 80, in this line, is life to be; that at zero is life past. The increase of life which is effected by the operation of the exciting powers, is expressed by decreasing numbers from 80 to 40, in the line of excitability; and by increasing numbers from zero to 40 in those of exciting powers and of excitement: 40, therefore, which corresponds in all the three lines, is marked as the point of life in its completest state, and most perfect vigour. Beyond this the decay of life, which is also effected by the operation of the exciting powers, is expressed by decreasing numbers from 40 to zero, equally with respect to excitability and excitement, the exciting powers continuing to increase to 80 (*loc. cit.*).

With respect to this scale, considered as a scale of life in health, it may be observed, *first*, that if the numbers in the first line can be supposed to represent with any tolerable degree of accuracy, the quantity and force of the natural exciting powers, such as heat, air, food, &c., necessary to produce in each successive period of life the degree of excitement in which health consists, it would follow that a man at 60 years of age must require, for the maintenance of his healthy state, three times as much of these stimuli as a man at 20 years of age requires, in order to produce that degree of excitement in which health consists at his period of life. *But* how inconsistent this is with the fact, it is needless to remark.

*Secondly*, that as, according to this scale, "one de-



gree of exciting power applied takes off or destroys one degree of excitability, and every subsequent degree impairs the excitability in a proportion exactly equal to the degree of its force," after the excitability has been reduced to 40, there cannot possibly remain a sufficient quantity of this power to admit of the action of the stimuli supposed to be applied. For, as this scale has been constructed, while the excitability falls from 40 to zero, the exciting powers are continually increasing, so that at every degree beyond 40 the exciting powers are obviously more than sufficient to extinguish the excitability that remains, and this disproportion constantly increases to the end of the scale.

And, *thirdly*, that as, according to a principle stated by Dr Brown himself, the state of excitement in health should always be in the ratio compounded of the force of the exciting powers applied, and the degree of the excitability upon which they act; that state ought to have been represented in this scale, throughout every period of life, by the number 40, and not have been made, as has been done, a variable quantity.

Scale No. II. has been given as a representation of the deviations from perfect health to the extremes of disease. "The excess of excitement is contained between 40, the point of health in the table, and 70. The cases of indirect deficiency, or debility, are comprehended between 70 and 80. The cases of direct debility are all the degrees below 40. Betwixt 40 and 55, consists predisposition to Sthenic; and betwixt 40 and 25, the predisposition to Asthenic diseases." (Elements, § 82, Notes A and B.) From the inspection of this

scale, it seems obvious, *first*, that though the diminution of excitability goes on as in scale No. I., from 80 to zero, yet the degree of excitement produced by the operation of the exciting powers upon the decreasing excitability beyond 40, is represented by increasing numbers from 40 to 80 in the line of excitement, instead of decreasing after attaining that middle point as in scale No. I., without any reason being assigned for this difference in the amount of excitement produced by corresponding degrees of exciting powers and of excitability in the two scales of health and disease.

*Second*, That the exciting powers which are in this scale to be considered as hurtful or as remedial powers, as powers producing or removing diseases, are represented as producing a degree of excitement uniformly correspondent to the force in which they are applied, and totally independent of the state of excitability on which they operate.

*Third*, That the exciting powers, in producing increasing degrees of excitement beyond 40, have a gradually diminishing excitability to act upon : for example, 60 degrees of exciting power acting upon 20 degrees of excitability, are represented as producing 60 degrees of excitement ; 70 degrees of exciting power, acting upon only 10 degrees of excitability, produce 70 degrees of excitement ; and, what is still more inconceivable, 80 degrees of exciting power, acting upon an excitability represented as extinct, produce no fewer than 80 degrees, or the highest possible degree of excitement !

*Fourth*, That in scale No. I. the excitability is supposed to be gradually diminished in the progress

of life from 80 to zero, by the continued and increasing force of the natural exciting powers; from birth to 80 years of age; for example, one degree of excitability is annually wasted by the action of the natural exciting powers; and year after year, one degree is regularly wasted in this manner, till the sum-total of 80 degrees has been gradually consumed by the continued action of these powers, and death, in consequence, has ensued. But in No. II., considered as a scale of disease, it is supposed by Dr Brown that the excitability may, in an instant of time, be accumulated from 40 to 80, by the abstraction of stimuli, and as suddenly wasted, by their addition, from 40 to zero, on the other side of the scale. "A," says Dr Brown (Elements, § 71, Note *d*), "is affected with a disease of debility, where the excitement has gone down to 10, the excitability mounted up to 70 degrees in the scale. By a proper use of high stimulants, the 30 degrees of lost excitement may be restored, and as many of superfluous excitability discharged, and the excitement and excitability made to meet again at the middle point of 40. If the remedies are carried up to any degree betwixt 40 and 55, they have gone too far, and produced predisposition to Sthenic diseases; if still farther, but not exceeding 70, they will have produced one or other of these diseases. But carried beyond 70, the diseases which their operation produces are those of Indirect Debility. Any disease of this sort, when treated according to rule, will be cured. But if the stimulants adapted to this purpose be urged further, the Sthenic diathesis will again be produced; and the debilitating power suited to the removal of it may carry down the excitement below 40 into the range of predisposition betwixt 40 and 25; and then, by a further abuse of remedies, which should only be used in the range betwixt 40 and 70, the excitement may return to the same point from

which it set out, to-wit, the point of 10, and the excitability rise to its original point, that of 70." In the construction of this as well as of Scale No. I., it is supposed that a person who has arrived at the middle period of life has had 40 degrees of the sum-total of his excitability wasted, and consequently that he can have only 40 degrees more left to carry him through the remainder of life. Yet, according to the doctrine taught in the above quotation, a person in this condition can, by the application of debilitating powers, have his excitability brought back from 40 to 70. How such an accumulation of excitability can in these circumstances be effected, it is difficult, if not impossible, to comprehend; Dr Brown having made no provision, as has already been remarked, for any fresh production of that principle, the sole phenomenon occurring in the animal economy being, according to him, the waste of excitability by the action of exciting powers. Surely this must have been one of those points in the Brunonian theory, of which we are informed by Dr Brown himself, in a note (Outlines, § 23), that "the author is just now revolving in his mind an alteration in some of the terms of the fundamental proposition, which may, in time, end in a different mode of expressing it."

*Fifth*, That Scale No. II., considered as a scale of disease, can be applicable only to a person who has arrived at the middle period of life, when the excitement is at 40 degrees, "the point of life in its completest state and most perfect vigour." According to the representation given in this scale, there can be no diseases of increased excitement before the middle period of life, and none, consequently, requiring the abstraction of stimuli; while in the decay of life, from 40 to 10

in the line of Excitability, the diseases which occur must be those of excessive vigour, requiring for their cure the abstraction of stimuli ; and beyond this, from 10 to zero, there can occur only diseases of Indirect Debility, which require the use of the most powerful stimuli. Daily observation, however, shews, that in every period of life, from infancy to old age, men may enjoy the most perfect health ; and likewise that diseases of increased and of diminished excitement may and do occur in all the stages of human existence. Dr Brown, it is true, has said (*Elements*, § 26), that “every age, and every habit, if the excitement be properly directed, has its due degree of vigour accommodated to it ;” but he has left us in total ignorance with respect to the degree of excitement which constitutes the standard of health in any period of life, with the single exception of middle or adult age.

It seems by no means improbable that the source from which Dr Brown derived his idea of a scale of health and disease, was certain observations made by Dr Cullen in his lectures on Pathology, and which have recently been quoted (see pages 249 and 251), on the standard and the latitude of health, and on the difficulty of fixing precisely the application of the terms of Excitement and Collapse. In a scale constructed in conformity with Dr Cullen’s suggestion, health in all periods of life would have been adopted as the middle point ; and the deviations on either side, consisting in different states of Excitement and of Collapse (or, in other words, of over and under-excitement), passing through the latitude of health to disease, would have been represented by degrees ascending and descending from that middle point. Whether Dr Brown has improved

upon the suggestion of Dr Cullen, and how far the representations given in his scales of health and disease are reconcilable with one another, consistent with the opinions which they are intended to illustrate, or calculated to assist us in the practice of medicine, must, with these observations, be left to the judgment of the reader to determine.

XI. “Conformably with the division of diseases into Sthenic and Asthenic, there can be only two kinds of Remedies, which always restore the healthy state by opposing deficient stimulus to excessive, and excessive to deficient. The hurtful exciting powers which produce Sthenic diseases are the remedies of Asthenic, and those which produce Asthenic, are the remedies of Sthenic diseases. The same debilitating powers which cure any one Sthenic disease, cure every one; the same stimulant powers which remove any Asthenic disease, remove them all.” (Elements, § 64, 23, 89. Outlines, § 85, 117.)

It has been already observed, that, according to the principles of the Brunonian theory, all the agents which are capable of acting upon the living body have one common operation, namely, that of producing excitement, or of stimulating, and differ from one another only in the degree in which they produce this effect. The terms Stimulants and Debilitants, therefore, as applied by Dr Brown to his two classes of remedies—those which increase, and those which diminish excitement—must be considered as expressive, not of different kinds of agents, but only of agents producing different degrees of the same mode of action.

Dr Cullen may be said to have prepared the way for this view of the relations of Stimulants and Debitants, by having remarked in his Lectures on Therapeutics, that "different aliments are of different degrees of stimulant power, and the substituting a weaker in place of a stronger stimulus, is the applying of a sedative to our system." From this quotation it is obvious that the peculiarity of Dr Brown's therapeutical doctrine does not consist in his regarding substances of feeble stimulating powers as Debitants, for that had been a common belief, but in his denying that there exist any direct sedatives, or primarily debilitating powers in nature. It seems easy to understand, upon his theory, how the taking away stimuli altogether, or the substitution of a weaker for a stronger stimulus, should diminish excitement; and it seems easy, also, to understand how the excessive use of stimuli should ultimately be followed by the same effect. But, it does not seem to be explicable, on the principles of the Brunonian theory, why, during the application of the ordinary supporters of life, the introduction into the general system of particular poisonous substances, or of animal contagions, should, either immediately or after a short lapse of time, be followed, as it frequently is, by a great, perhaps even a fatal, diminution of excitement. If these agents are possessed of only very feeble exciting powers, their application will not be sufficient, independently of the ordinary supporters of life, to prevent that diminution or extinction of excitement which arises from deficiency of stimulant powers, but it should not have any influence in producing or accelerating these states;

and if, on the contrary, we are to believe that these agents possess a high degree of stimulant power, how does it happen that they so frequently prove fatal, without occasioning any marks of that increased excitement, through which, according to Dr Brown's theory, the economy must pass, before death can be produced by the total exhaustion of excitability?

The various therapeutical agents or remedies that are capable of altering or removing the different morbid conditions of the human economy, functional and structural, have very commonly been divided into those which act on the general economy, and those which act only on particular parts of it. By remedies acting on the general economy, medical men seem to have meant chiefly to designate those which act more immediately on the Sanguiferous and Nervous Systems, and which, in consequence of the reciprocal influence of these two systems on one another, and of the dependence of the different organs of the body upon them, come speedily to affect, in a greater or less degree, the condition of the whole economy. By remedies which act on particular parts of the economy, again, are meant those, the primary and principal operation of which seems to be on some of the subordinate organs of the body, and which affect the sanguiferous and nervous systems, and, through them, the other parts of the economy, only in a secondary or indirect manner, or in a less considerable degree.

The states of increased and diminished action or excitement, on which alone Dr Brown conceived all diseases to depend, had long been regarded by medical men, and had particularly been pointed out by Dr



Cullen, as the most frequent morbid conditions to which the different systems and organs of the body, and more especially the organs engaged in circulating the blood, are subject in pyrexial diseases ; and those remedial powers, therefore, which diminish excessive, and increase deficient, action of the sanguiferous system—whether they belong to the class of remedies supposed to act on the economy generally, or to those which act on its particular parts—had, as we shall afterwards have occasion to shew, been justly considered as amongst the most important therapeutical agents. But to the view which Dr Brown has taken of these two morbid conditions as applicable to all diseases, and of the corresponding remedial powers, two obvious and fundamental objections present themselves ; the first, that it is impossible to maintain with regard to any single disease, and far less with regard to any class of diseases, that, in all the various circumstances in which that disease, or class of diseases, may present itself, it should be treated solely upon stimulant, or solely upon debilitating, principles ; and the second, that remedies do not by any means admit of being all included under the two classes to which Dr Brown has referred them.

1. In judging how far all diseases admit of being divided, as has been done by Dr Brown, into two distinct classes, according as they require a stimulant or a debilitant mode of treatment, it is necessary to keep in mind how much variety, in regard to the states of vigour and debility, a particular case of the same disease may exhibit, both at any given period of its progress in the different functions that may

be morbidly affected, and in its successive stages in the same functions. In a case of disease, in which the organs performing the different functions of the economy, or even those performing different parts of the same function, are in different or in opposite states of vigour and debility at the same instant of time, it is obviously not to the excitement over the whole economy, that is to say, not to the action of the whole of the functions collectively, but to the particular state of the individual functions that our indications of cure must be directed. In how many cases of fevers and of nervous affections, is it necessary to apply stimuli to one part of the body, as, for instance, heat to the feet, and, at the same time, to abstract stimuli from another, as by the application of cold to the head, or by the detraction of blood from the general system? "I must observe," says Dr Cullen in his Lectures on the Practice of Physic, in speaking of Palsy, "that there are some stimuli which act on the nervous system, without affecting the heart and vessels in any degree; and others which act powerfully on the sanguiferous system, without having any great apparent influence upon the nervous system. Hence I would divide stimuli into Nervous and Inflammatory: but these are not quite disjoined,—Nervous stimuli cannot be long applied without bringing the heart and vessels into consent, and *vice versa*."

In like manner, if the condition of particular functions in respect of vigour and debility, may vary much in the successive stages of the same case of disease, it is obvious that corresponding changes will be required in the character of the remedies employed in these successive stages, according to the predominance of the one or other of these states. A case of disease which,

in its first attack, does not admit of the use of general stimuli, may in its progress come to require these remedies ; and, on the other hand, every practitioner of experience knows that in a case of disease in which the patient would sink in the first stage but for the liberal administration of stimulants, it may be necessary not only to cease from the use of that class of remedies as soon as the vigour of the economy, particularly as indicated by an increased action of the heart and arteries, has been in some measure restored, but even to have recourse to debilitating remedies, if the vigour should, as frequently happens, become morbidly excessive.

But if, in respect of individual cases of disease, it would be a hopeless attempt to distinguish them *pathologically*, according as they are cases of increased or cases of diminished excitement, or *therapeutically*, according as they require a stimulant or debilitant plan of treatment ; equally, or even more futile must be the attempt to apply such grounds of distinctions and divisions to particular diseases, for it is well known that almost every disease exhibits great variety as it occurs in different individuals, in respect of the vigour or weakness with which the several functions, and, in particular, that of the sanguiferous system, are performed at given periods of its progress ; and the practitioner is consequently compelled to be guided in the stimulant or debilitant nature of the remedies he employs, not by the mere name which the disease may bear, or by the place which it may hold in a pathological or therapeutical arrangement, but by the character of the symptoms which it shall exhibit in concurrence and in succession, in each of the different individuals who may be affected by it.

If such be the diversities, in regard to the states of increased or diminished excitement, which the same case of disease may exhibit in the different functions, vital, animal, and natural, at the same period, or in the same function at the different periods of its progress; and such the diversities, in these respects, which the same disease may present in the different stages which it passes through, and in the different individuals whom it attacks, though living at the moment of attack in the same circumstances, and apparently in equally sound conditions of health; how can it be supposed possible to deduce, *a priori*, from these states an universal character of any disease? or what results could reasonably be expected from a plan of treatment not specially adapted to the particular circumstances of the cases in which it is pursued, but proposed on the erroneous supposition that a disease, in all the circumstances in which it may occur, must necessarily, from the identity of its cause, assume precisely the same character, hold precisely the same course, and be combated by precisely the same remedies? Is it not from having learned by experience how frequently the states of increased and diminished excitement of the vascular system subside without the employment of remedial measures, and how liable even each of them is to be succeeded by the opposite state, that the judicious practitioner is so very reluctant to interfere with the spontaneous course of diseases in which either of these states prevails in a moderate degree; and that even when, from the severity of the symptoms, he considers such interference to be necessary, he is so particularly cautious not to carry the stimulant

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or debilitant measures which he employs, farther than may be requisite for removing the danger more immediately impending?

2. As there occur many morbidly altered states of the different solid and fluid parts of which the body is composed, that do not seem to be strictly referrible either to increased or to diminished excitement, so, in the action of many remedies by which morbid conditions of the different textures, systems, and organs are removed, we are unable to discover any evidence of their increasing or diminishing the excitement either of particular organs or of the general economy. The experience of practitioners in all times and places has shewn that the diminution of excessive and the increase of deficient, action, are very far, indeed, from comprehending all the therapeutical indications which diseases afford; though Dr Brown has affirmed that universal diseases admit of no other indication of cure. It is in the different forms and stages of febrile and inflammatory diseases, as has been so distinctly pointed out in the lectures and writings of Dr Cullen, that the states of excessive and deficient action, particularly of the sanguiferous system, furnish the principal indications of treatment. In by far the greater part of nervous and cachectic diseases—the symptoms of which consist chiefly in irregular actions of the organs subservient to the animal and natural functions,—these two indications of treatment are comparatively of but rare applicability.

Hoffmann, in conformity with his pathological division of diseases into those of Spasm and Atony, or excessive contraction and dilatation, suggested as the foun-

dation of therapeutical indications, that "since there are only two kinds of morbid motions, the intense and the depressed, it very plainly appears that there are only two kinds of remedies to be established ; one set of which is intended for allaying (sedandis) pathic motions, the other for restoring tone to the parts that are flaccid. The one set, according to the phraseology established in the schools, are called Antispasmodics and Sedatives, the other Roborants and Tonics." (Medic. Ration. Systemat. Præfatio, p. 8. See also Lecaze Institut. Med., p. 209.)

Dr Cullen had remarked in his lectures on the *Materia Medica*, delivered in 1761 (4to edit. p. 361, and *Emendanda*, p. 18.), that "the diseases of the Moving Fibres are very various, but, taking a general view of them, we may," he says, "reduce them to three kinds ; 1st, Where contractility and motion are diminished. 2d, Where they are too strong, or too much increased. 3d, Where there is *irregularity* of motions. In the first case, stimulants are indicated, viz. such medicines as excite more vigorous contractions. In the second case, sedatives are indicated, by which term I mean those medicines, in whatever manner they act, which diminish too great contractility and motion. In the third case, antispasmodics are indicated, under which term, to avoid cavil, I mean such medicines as compose, or take off, *irregular* motions in our system." "Some have thought," he adds, "that all antispasmodics are either sedative or stimulant ; but besides those which are more obviously either the one or the other, there seem to be antispasmodics distinct from both. Stimulants are very generally such to the sanguiferous system, and very often sedatives shew the same effects ; but there are antispasmodics which do not at all. Again, there are antispasmodics which discover none of the narcotic qualities of seda-

tives ; and, therefore, from both considerations, we presume that there are antispasmodics distinct from both stimulants and sedatives. We shall indeed have occasion frequently to say that the antispasmodics are intimately connected with these other classes ; but still, we do not allow this to be universal, and would rather assert, that most of the medicines in our list of antispasmodics are more such than in proportion to their stimulant or sedative properties."

The therapeutical theory of Dr Brown obviously does not admit of any distinction being made between stimulant remedies, and those which have usually been denominated tonics. It may be remarked, however, that, conformably with Dr Cullen's views, the operation of tonics upon the system is quite different from that of stimulants, inasmuch as it is not accompanied by any of those sensible marks of increased force and frequency in the action of the circulatory organs to which the use of stimulants gives rise. (First Lines, § 217.) Indeed, though both of these classes of remedies have usually been considered as exerting their action on the general economy, yet a slight attention to the phenomena which they respectively produce is sufficient to shew that, whilst stimulants act more immediately and especially on the sanguiferous system, the primary operation of tonics is on the digestive organs, and particularly on the stomach.

As a farther objection to the division of all remedies into two classes according as they are stimulant or debilitant, it may be observed that some of the remedies comprised under the title of Local Stimulants or Evacuants, such as emetics, purgatives, diuretics, and sudorifics, are frequently employed in the treatment of diseases of excessive excitement, on account of the debili-

tating effects which they indirectly produce upon the general economy, or, more properly speaking, perhaps upon the sanguiferous system; and in this point of view, they may be considered as constituting a part of the Antiphlogistic or Antisthenic plan of cure. But the medicines of this class are often employed, also, in diseases that depend more immediately on derangements of some particular organ, for the sake of the effect they are capable of producing upon the organ or organs primarily affected, and without any view to their either increasing or diminishing the excitement of the general economy. Thus, several of the most powerful diuretic and cathartic medicines, which occasion the absorption and evacuation of the serous effusion in dropsical affections, exert little, if any, direct influence on the state of the sanguiferous system.

Nor does Dr Brown's twofold division of remedies take into account the morbid conditions of the blood, and of the different fluids secreted from it, which, though they occupy a much less prominent place in the pathology of the present than in that of former times, furnish several indications that have no connection, so far as can be perceived, with excess or deficiency of excitement, but that are of much importance in the treatment of the diseases in which they occur. The advantages derived from the use of lemon juice and of fresh vegetables in scurvy, of alkaline remedies in acidity of the stomach, and of both acid and alkaline remedies in calculous affections according to the chemical condition of the urine from which the calculous depositions take place, certainly do not admit of being referred to any influence which these remedies



exert in increasing or diminishing the excitement of the general economy.

After these remarks, it is perhaps superfluous to add, that the very general and imperfect view which Dr Brown took of the action of remedies on the various morbid conditions of the animal economy, in regarding them as differing from one another merely in the degrees of their stimulant power, was in nowise calculated to promote the science of the *Materia Medica*, and forms, in this respect, a most remarkable contrast with those comprehensive results of practical observation and analytical discrimination that had been exhibited by Dr Cullen in his lectures on that subject and on General Therapeutics.

But the claims which Dr Brown advanced to originality, with regard to the treatment of diseases, were not limited to his having simplified the practice of the art, by reducing all curative indications to the two heads of Stimulating and Debilitating. He does not scruple to lay claim to the further merit of having been the person by whom these two general plans of treatment were invented. In speaking of his plan of treatment for asthenic diseases, Dr Brown affirms (*Outlines*, § 114), that “the stimulant plan of cure, whether the theory or the practice, the cause, the exciting powers, the indication of cure, or the remedies, be considered, is, in all its parts, *brewnew* ;” and with regard to the debilitating or antiphlogistic plan of treatment, we are told (*Outlines*, § 112), that “he lays claim to the discovery of even the cure of Sthenic diseases, from these two considerations,—*first*, The reduction of the cure to its right principle, which is also the common one of all the rest

of his doctrine ; and, *second*, The enlargement of it, so as to render it adequate to all the purposes of practice in every possible case." A few observations will suffice to shew whether there be any foundation for these pretensions. And, *first*, as to the Stimulant plan of treatment.

It would, indeed, have been a most remarkable fact in the history of medicine, if the debility or weakness, so frequently occurring in the progress of diseases, or if the means by which this state can be best alleviated or removed—which are at all times objects of so much concern to the sick and to their friends—had, till the time of Dr Brown, escaped the notice of medical men, or had been passed over by them as undeserving of attention ; if, in short, as he asserts to have been the case, physicians had had, up to his time, "no idea of any other morbid state but a phlogistic one, or one depending upon excessive vigour, and no conception of any other mode of cure but an antiphlogistic debilitating one." (Outlines, page 50, Son's edition.) So far, however, from this being the fact, it would require very little research into the writings of those physicians for whose labours Dr Brown expresses so much contempt, to satisfy any candid inquirer, that not only has the state of debility claimed an important share of the attention of medical men in all periods of their art, but that, in the opinion of the most judicious practitioners of all ages, there has been a disposition to carry the use of the remedies calculated to remove this state, to an excessive degree. The perusal of the twelfth book of Galen's *Methodus Medendi* might have sufficed to convince Dr Brown, that neither the gene-

ral causes of debility, nor the beneficial effects of wine and other stimulants, in the treatment of diseases attended by that state, were unknown to the ancients. Even anterior to the time of Galen, he might have found an example of a mode of treating diseases not very dissimilar to that of which he conceived himself to be the inventor, in the practice of Petron, who, as we are told by Celsus, administered roasted pork and wine to patients in convalescence from slow fever. This practice, Celsus remarks, was not less acceptable to those whom the successors of Hippocrates had failed to cure, than it was to those, in his own age, who had been long unsuccessfully treated by the followers of Herophilus or Erasistratus. "Nor is this kind of practice," he adds, "though popular, not to be esteemed rash ; for, if pursued from the commencement of diseases, it kills more than it cures."

One who, like Dr Brown, arrogated to himself the credit of having presented the public with an entirely new science of medicine, might have been expected to have had some acquaintance with the writings of those whose pretensions, at least, whatever he might think of their performances, had not fallen short of his own. Were we to trace back the list of physicians who had preceded Dr Brown, we should probably not find any one intervening betwixt him and Van Helmont, who imagined himself to have introduced such wonderful reforms in the science of medicine ; and if Dr Brown had been acquainted with that author's treatise, entitled *Februm Doctrina Inaudita*, in which he informs the reader that "*Nemo hactenus febres ex essentia novit, nemo illarum sanationem ex arte instituit,*" and had

perused his arguments against the use of bloodletting and purging (chaps. iv. and v.), and in favour of the administration of wine, in these diseases (chap. xii. § 7), he could scarcely have had the hardihood to assert that these were improvements in the practice of physic entirely original.\*

In times nearer to our own, Sydenham, in alluding to the stimulating practice followed in fever by some of his contemporaries, has observed (*Schedula Monitoria*, § 41), that “The invention of the term, or opinion, Malignity, has been far more destructive to mankind than the invention of gunpowder. For as those fevers are principally entitled Malignant which are found most inflammatory, hence it is that physicians have recourse to certain cordials and alexipharmics, in order to expel the imaginary poison by the pores; and, upon the same foundation, they have adapted the warmest regimen and medicines to those diseases which chiefly required the reverse.”†

A slight examination of the writings of Willis, Morton, and Baglivi, of Hoffmann and of Boerhaave, will

\* Scuderi, in comparing Dr Brown's division of diseases with that of the Methodic School, and in referring to Galen's refutation of that division, has designated him the “new Scotch Thessalus.” The want of candour with which Thessalus represented as his own, doctrines which he had derived from his predecessors; the contempt he expressed for all that had been done by the physicians who preceded him; the exalted opinion he entertained of his own merits and of the perfection of his system; and the simplicity to which he affirmed that he had reduced both the study and the practice of physic, are all traits of character which, fully as much as the resemblance of the Methodic and Brunonian doctrines, justify Scuderi's comparison. (See Galen's *Methodus Medendi*, lib. i.)

† See also Baglivi, *Prax. Med.* lib. i. § 9.

sufficiently prove, that however much these physicians may have differed from one another in the theoretical views by which they were guided in the administration of stimulant remedies, and as to the extent to which the use of these remedies should be carried in the treatment of particular diseases, and of individual cases of the same disease, they were far from neglecting their use. Indeed, so long as the natural inclinations of patients, and the well meaning, though often mistaken, kindness of attendants, shall continue to operate, there never will be wanting admirers and imitators of the practice of administering, however indiscriminately and improperly, a stimulating diet, with wine and other cordials, in the treatment of all forms of diseases, and more particularly in the convalescence from diseases which, during their continuance, had required a reducing plan of treatment, or a long-continued abstinence from animal food.

But it was not necessary for Dr Brown to recur to authors previous to his own time to find a complete refutation of his assertion, that he was the first who had pointed out the state of debility as deserving the attention of medical practitioners, and who had proposed a plan of treatment for its cure. In the lectures of Dr Cullen, with which he had enjoyed so particular advantages in making himself acquainted, there is abundant evidence of the absurdity of these pretensions. In those on *Materia Medica*, delivered in 1761-62 (p. 240), Dr Cullen remarks, in regard to stimulants, that they "are indicated in all cases of languid motion; that is, not merely in weakness, but in all cases where the motion of the system is inert, weak, and

slow ;” and then proceeds to consider them successively as indicated with respect to the system of blood-vessels, with respect to the nervous system, and with respect to the alimentary canal. It was in his lectures on the Practice of Physic, however, in treating of fever, that Dr Cullen took occasion to enter at greatest length into the consideration of the symptoms of debility, and of the modes of overcoming that state. The 104th paragraph of his First Lines, along with the commentary which he was accustomed to deliver on it in his lectures (Works, vol. i. p. 572), contains an enumeration and exposition of the symptoms of debility, as they occur in the vital, animal, and natural functions, more complete, probably, than any that had been given since the publication, in 1601, of Prosper Alpinus’s work *De Præsagienda Vita et Morte* ; a work which Dr Cullen seems to have studied with much attention, and to which, in his lectures, he frequently referred. In considering the treatment of fevers, Dr Cullen has laid down, as his second indication of cure, “ To remove the causes or obviate the effects of debility ;” and certainly the account which he has given of the means by which this indication may be best fulfilled, from paragraphs 201 to 220 of his First Lines, could receive neither useful addition nor improvement from Dr Brown’s asthenic plan of cure. It was not, however, when treating of fevers alone, that Dr Cullen directed his attention to the state of debility as occurring in diseases, and to the stimulant plan of cure ; for in treating of the other genera of the class *Pyrexia*, he was at all times careful to notice the typhoid form which their febrile symptoms may assume, and to point

out those modifications in the plan of treatment which the occurrence of the symptoms of debility may render necessary.

With respect to the improvements which Dr Brown alleges he has introduced into the Antiphlogistic plan of treating diseases, he seems desirous that it should be understood that these consist in the use of emetics and sudorifics, in enjoining abstinence, and rest of body and mind, and in a more extensive use of cold as a remedy in diseases, than had previously been made by any other practitioner; for he alleges (Elements, § 306), that "the cure of sthenic diseases has hitherto consisted in bleeding, purging of the belly, and in the use of refrigeration in a few cases, while the other objects of attention here so fully considered, have either been totally neglected or mentioned in a slight way, by-the-by, and as if they had been of no consequence, and in the cures which were prescribed in that way, not reduced to any principle." In making these assertions, Dr Brown could not but be fully aware that Dr Cullen had considered the subject of the antiphlogistic regimen in detail on two occasions; *first*, in his lectures on Therapeutics, when treating of sedatives (see vol. i. p. 421); and, *second*, in his First Lines and lectures on the Practice of Physic, when discussing the first indication which he has laid down in the treatment of continued fever, viz. that of moderating the violence of reaction. (Works, vol. i. p. 599.) Whoever will take the trouble to compare these two articles with Dr Brown's plan of treatment for Sthenic diseases, will find, that not only bloodletting and purging of the belly were enforced by Dr Cullen as parts of the antiphlogistic re-

gimen, but that the particular circumstances in which emetics and sudorifics should be had recourse to, were carefully and judiciously pointed out by him, and the necessity of abstinence from stimulating food, and of avoiding exercise of mind or body, strenuously inculcated; and he will find there, also, the source of those opinions respecting the more extensive employment of cold, as a part of this mode of treatment, which Dr Brown has brought forward with so much pretension to novelty.

As all diseases, according to Dr Brown, are caused by excess or by deficiency of the exciting powers, and are to be "removed by an action of the remedies, the same in kind, but opposite in degree, to that which produced them" (Elements, § 89), it might be supposed that the object to be had in view in the treatment of diseases must always be to employ stimulant remedies where they arise from deficiency, and debilitating remedies where they arise from excess, of exciting powers. In this respect, however, the theoretical opinions and practical precepts of Dr Brown do not seem to be very consistent with one another; for though the use of stimulant remedies is, according to him, proper and necessary in all the diseases arising from deficient stimulus, yet the diseases arising from excessive stimulus do not all admit of the use of debilitants. Excessive excitement, he alleges (Elements, § 103), may proceed so far as to produce asthenic diseases of indirect debility, and in these, as well as in the asthenic diseases of direct debility, he conceives that stimulants are to be employed. (Ibid. § 682.) According to this doctrine, therefore, in a constitution in which



the excitability is, in the language of the system, nearly exhausted, we are not, as might have been expected from the terms of the general indication, to diminish the exciting powers to the lowest possible degree compatible with life, but to go on still farther wasting the excitability, by the employment of agents capable of producing a degree of excitement greater than that essential to health.

A slight inspection of the table, in which diseases are classed by Dr Brown according to the views he entertained of the degrees of excitement on which they depend, and of the general plan of treatment which they require, will be sufficient to shew that the practice recommended by him differs from that which had usually been employed, chiefly in this respect, that several diseases which, in consequence of the morbid state of the sanguiferous system that prevails in them, had been commonly considered to require, in the majority of cases and in most circumstances, an antiphlogistic plan of treatment, are, by him, referred to the class of diseases requiring the use of stimulant remedies. Thus, for example, epilepsy, apoplexy, and palsy, are regarded (*Elements*, § 627, 637, and 632) by Dr Brown as asthenic diseases arising from indirect debility, in which the indication of cure is to support the excitement by the use of powerful stimuli. Hæmoptysis is considered as a disease arising from direct debility, in which the indication of cure is to increase the excitement by the use of stimuli; and gout appears in Dr Brown's catalogue as a disease of extreme asthenic diathesis in which stimuli are indicated, though it is admitted that great caution is necessary in their ap-

plication. The notion of these diseases being diseases of debility, could only have arisen, in Dr Brown's conception, from his assuming as his standard of vigour the degree of force with which the voluntary motions can be performed, instead of that with which the blood is propelled through the heart and arteries. And it need not appear surprising that, by applying the same criterion of vigour to other diseases, and by his total neglect of, and his contempt for the experience of all other practitioners, he should have been led to the singularly absurd and dangerous conclusion, that, in the course of medical practice, not fewer than ninety-seven cases of disease out of a hundred require to be treated by stimulant remedies.

Dr Cullen had observed, in his lectures on the Institutions of Medicine, as had indeed been frequently done by other physicians before his time, that "the same agents which are upon one occasion the causes of disease, are, upon another, equally employed as the remedies of it. Thus, heat and cold," he remarks, "may be considered as remote causes of disease, but, upon innumerable occasions, we find that they are most useful remedies; and so with regard to other agents, according to the condition of the body to which they are applied." This observation of Dr Cullen's, in passing through the mind of Dr Brown, was, as usual, converted from a general to an universal proposition; and in that spirit of indiscriminating generalization which characterizes so many of his other doctrines, he affirmed, that the powers which produce the one of his two classes of diseases are reciprocally and universally the remedies of the other. But a comparison of the

lists of his two classes of diseases, those of excessive and those of deficient excitement, and of the remedies employed in their treatment, is all that can be required to shew the absurdity of this opinion. Is it possible that Dr Brown could for a moment conceive that the contagions by which the sthenic form of smallpox, measles, and scarlatina are produced, act as remedies in any diseases of an asthenic character ; or that we may hope to cure any of the diseases of the sthenic class by the marshy exhalations that produce intermittent fevers, by the salt provisions which produce sea-scurvy, or by the vapours of lead which give rise to colica pictonum ?

It appears to have been the very great importance which Dr Brown attached to the influence of food and drink in producing and in removing diseases, that led him to adopt, without any limitations whatsoever, the ancient therapeutical doctrine of diseases being cured by Contraries. Indeed, the science of medicine, both as regards the causes by which diseases are produced and the remedies by which they may be cured, seems to have resolved itself very much in his conceptions, as it has done in those of many of his followers, into a question of eating and drinking. An inadequate supply of nutritious food is with him the grand cause of diseases of deficient excitement ; for the cure of which the patient must, by a gradual transition, be brought to the use of a diet more than usually nourishing, and of a liberal allowance of stimulating liquors. Over-indulgence in the pleasures of the table, on the other hand, is the grand cause of diseases of excessive excitement, for the cure of which it will be necessary gradually to re-

strict the patient to a sparer diet, and to a smaller measure of vinous or spirituous liquors. These were the ætiological and therapeutical ideas which seem to have been ever predominant in Dr Brown's imagination, and by which he was chiefly guided in his views of practical medicine. In filling up the details of his system, he was led to the agreeable conclusion that, in an immense majority of instances in which men become affected with diseases, they have to blame, not their own intemperance, as physicians and moralists have usually concurred in endeavouring to persuade them,\* but their own abstemiousness, in denying themselves that allowance of nutritious food and of strong drink which is essentially necessary to maintain their bodies in a healthy condition.

XII. "The whole and sole province of a physician is to consider the deviation of excitement from the healthy standard, in order to remove it by the proper means. Such is the simplicity to which medicine is reduced by this system, that when a physician comes to the bedside of a patient, he has only three things to settle in his mind; 1<sup>st</sup>, Whether the disease be general or local; 2<sup>d</sup>, if general, Whether it be sthenic or asthenic? 3<sup>d</sup>, What is its degree? When he has once satisfied himself on these points, all that remains for him to do is to form his indication or general view of the plan of cure. And since every disease, and every predisposition to it, depends upon an increase or

\* "Cumque maxima pars morborum," says Baglivi, "ab immodica gula proficiscatur, erroribusque in sex rebus non-naturalibus," &c. Opera, p. 254.

a diminution of excitement, and is removed by the conversion of that into the middle state betwixt them, therefore, to prevent as well as to cure diseases, the practitioner must always either stimulate or debilitate, never desist from acting, nor trust to the supposed powers of nature, which have no real existence." (Elements, § 80, note *g*. 95. Outlines, § 88, 149, note *a*.)

The first object of the physician in investigating the nature of a patient's disease should be, according to Dr Brown, to ascertain whether it be general or local. Dr Cullen, in his Nosology, had divided diseases into General and Local—"those which affect the whole system, and those which affect one part of it only," as Celsus, indeed, had long before done in his Treatise on Medicine ; but he seems to have been at all times fully aware how often these two kinds of diseases coexist, and reciprocally give rise to, and influence one another. Dr Brown, in adopting this division of diseases, endeavoured to render it more precise than it had ever previously been, by pointing out determinate characters by which the two classes may be distinguished ; local diseases differing, according to him, from universal, "in the hurtful powers which produce them, in their true cause, in their cure, and in every essential respect ; agreeing with them in nothing but a deceitful and deceiving superficial appearance." (Elements, § 81, 83.) But how little foundation there exists in nature for making so strict a division of diseases into general and local, as that which Dr Brown conceived he had established, must be sufficiently manifest from the tendency which both these kinds of diseases have reciprocally to produce each other, as is proved by the very small number of dis-

eases in which general and local affections are found to be uncombined. Even those affections which are most strictly termed Local, as arising from the immediate application of external violence to some particular part of the body, are liable to become attended in their progress by marks of constitutional or general derangement; whilst in fevers, which have usually been considered as general diseases, evident marks of local affections—such as determinations of blood to the brain, to the lungs, or to the abdominal viscera—frequently occur, and prove a great source of danger. In these instances, there is no difficulty in determining which is the primary and which is the secondary affection, which the idiopathic and which the symptomatic disease; but in many diseases, as in the Phlegmasiæ, or inflammatory diseases, the local and the constitutional symptoms frequently appear so nearly about the same time, that it becomes difficult, if not impossible, to determine whether they should be regarded as standing to one another in the relation of cause and effect, or as being simultaneous consequences of a common cause.

Dr Brown, indeed, claims to himself the merit of having discovered that, in the Phlegmasiæ, the general or constitutional febrile symptoms for the most part precede the local affection, and never succeed to it. “Long,” says he (*Elements*, § 382, note *g.*), “before any part of this doctrine was discovered, when I was in search of certain facts respecting peripneumony and pleuritis, I discovered one for which I was not looking, of more importance than all the rest put together. It had been asserted by most systematics, and by all the nosologists, that the primary symptom in the phlegmasiæ was the inflammation of a part. I saw that was not true with respect to rheumatism, in which the general

affection or pyrexia often rages one, two, or three days before the sign of inflammation, pain, is perceived in any of the joints. I could also discern that, from the moment the pain and inflammation appeared in erysipelas or the rose, there was also the general affection equally conspicuous. In short, in no one of that set of diseases did the fact appear that the inflammation was primary, and the pyrexia, or affection of the whole system, dependent upon it. But as peripneumony was said, in Edinburgh, to be an exception, the detection I made equally disproved that," &c. Who the systematics and nosologists were whom Dr Brown had in his eye in making these assertions, it is not easy to conjecture, as there does not appear to be anything in the systems of Stahl, Hoffmann, and Boerhaave, or in the nosologies of Sauvages, Sagar, or Cullen, to justify them. In no one of the three diseases to which Dr Brown refers in support of his assertion, has Dr Cullen affirmed that the local symptoms always precede the constitutional. Of rheumatism he observes (First Lines, §441), that the pyrexia is sometimes formed before any pains are perceived ; but more commonly pains are felt in particular parts before any symptoms of pyrexia appear." In speaking of erysipelas as it attacks the face, he says (ibid. § 699), that after the pyrexial symptoms "have continued for one, two, or at most three days, there appears on some part of the face a redness, such as that described as the appearance of erythema." And with regard to pneumonia, he states (ibid. § 336), that "sometimes the pyrexia is, from the beginning, accompanied with other symptoms, but frequently it is formed for some hours before the other symptoms become considerable, and particularly before the pain be felt." Sydenham had previously said, in speaking of

pleurisy (Works, p. 227), "It generally begins with a chilliness and shivering, which are followed by heat, thirst, restlessness, and the other well known symptoms of fever ; in a few hours—though this symptom sometimes comes much later—the patient is seized with a violent pungent pain," &c.

Another opinion respecting the phlegmasiæ which Dr Brown represents as originating with himself, and for which, likewise, he seems to have taken great credit, was, that the local inflammation is not to be regarded as the cause, but merely as a part or symptom of the general disease. "The inflammation which accompanies the phlegmasiæ," he remarks (Elements, § 169, 170, 168, H), "is nothing else but a state of the inflamed part, of a common nature with that in the rest of the body. And as the inflammation is produced by a greater degree of excitement in the inflamed than in any other equal part, so, before the disease comes on, of which the inflammation is a part or symptom, the excitement of that part is understood to be proportionally greater than in any other part. The degree of inflammation which is a symptom of general sthenic diseases, and which, for the sake of distinction, is to be called General Sthenic Inflammation, is proportioned to the degree of the sthenic diathesis." The perusal of the following extracts from Dr Cullen's First Lines and from his manuscript lectures on the Practice of Physic, will, it is conceived, leave but little doubt as to the source from which Dr Brown's opinion respecting the dependence of the local upon the general affection in the phlegmasiæ had been originally derived. In allusion to the Diathesis Phlogistica (First Lines, § 247), Dr Cullen observes, "Such a state of the system seems often to arise and subsist for some time, without the apparent inflammation of any particular part; but such a state of the system renders it



likely that a spasm may, at the same time, readily arise in any of the extreme vessels, and a particular inflammation be there produced. It does, however, appear also, that the general diathesis frequently arises from inflammation begun in a particular part." In pointing out, in his lectures, the various ways in which the resolution of peripneumony may take place, Dr Cullen makes the following observation : " There is a curious supposition of Dr Boerhaave's, that the morbid matter causing pleurisy or peripneumony is, upon many occasions, translated from the lungs to other distant parts of the body, and discovers itself by inflammation and abscesses in these parts. This at least deserves to be taken notice of in this way, that the chief circumstance in the phlegmasiæ is not the topical affection, but the phlogistic diathesis that is communicated to the system. And it may be a question whether or not this diathesis phlogistica is not the fundamental disease, and its affecting particular parts an occasional and accidental circumstance ; and we know that its determination to one or other part of the body is merely accidental and various. Dr Cleghorn, in treating of peripneumony, gives a variety of instances where the inflammatory affection of the lungs is accompanied with inflammation of the brain, angina, various abdominal inflammations, and various erysipelatous and phlegmonic inflammations of the external parts, and he gives instances of their shifting from one part to another ; and he particularly applies this to the case of peripneumony, that sometimes the disease passes from one lobe of the lungs entirely into that of the other." In speaking of the general state of the system occurring in rheumatism, Dr Cullen observes, " The general diathesis produced—the phlogistic diathesis prevailing over the whole system, which, I have said before, consists in an increased tone of the whole arterial system, may be imputed to topical *irritation* that has been communicated to the rest of the system ; and very often arises from thence entirely,

and from what is a topical affection. But from whatever it may have arisen, it may truly be considered as subsisting very much independently of the topical affection. It is very necessary, with a view to practice, to mention this. In the first place, it explains so well the combination of the several phlegmasiæ at the same season, which shews that there exists a predisposition, and that accidental circumstances determine this to certain parts more than to others. And it explains further why, in so many phlegmasiæ, the topical affections are so liable to change their place. I read a passage from Dr Cleghorn, in which he explains why a peripneumony or pleurisy that first attacks one lobe of the lungs, is very often, in the course of the disease, translated to the other ; and is not only translated to the different portions of the lungs, but is changed to angina, phrenitis, and various inflammatory affections in the other parts of the body. And nothing else will explain what is still more remarkable in rheumatism, that is, its frequent change of place, and that even when there is not what we may call a topical cause applied to produce those changes, the translation happening when the person is guarded against every thing of that kind. These facts undoubtedly serve to prove this, that the phlegmasiæ depend more upon the general diathesis than upon topical affection, at least in the case of rheumatism, which must influence our practice."

With respect to the second and third objects mentioned by Dr Brown as requiring the attention of the physician in investigating the nature of a patient's disease, viz. Whether it be of a sthenic or of an asthenic character ? and, What is its degree ? it must be allowed that the two states of vigour and debility, or of sthenia and asthenia, in whatever diseases they occur, must be objects of primary importance to the practitioner. They had always been regarded as such from

the earliest periods of the medical art ;\* and by Dr Cullen, in particular, they had been fully recognised and described, under the various terms of Increased and Diminished Action, Excitement and Collapse, Reaction and Debility, as different and opposite morbid conditions of the animal economy, furnishing two of the most important general indications to be fulfilled in the treatment of diseases. To these topics, however, it is unnecessary to revert, as it has already been shewn that the account which Dr Cullen gave of the phenomena of increased and diminished excitement, as occurring in fevers and in other general diseases, and of the plans of treatment indicated by the predominance of the one or other of these states in the commencement and progress of diseases, was the source from which Dr Brown derived his description of the sthenic and asthenic diatheses, and of the sthenic and asthenic plans of treatment ; and it has been shewn also, it is conceived, that Dr Brown's attempt to refer all diseases to one or other of these two states, and to reduce the whole of medical practice to two general plans of treatment, is founded on an imperfect enumeration, and too exclusive a generalization of morbid states ; there being many diseases in which it is impossible to refer the symptoms that occur to the presence either of increased or diminished excitement ; and others, in which, though slight marks of one or other of these states may occur, salutary changes take place spontaneously without the aid of art.

*sum in cura momentum illud est, quod vires aegroti  
pœa enim vita ex harum est observatione. Vis vitæ,  
bus est consideranda." (Galen, Methodus Medendi.)*

In judging of the presence of the states of vigour and debility in the constitutions of individuals affected with diseases, and of the degree in which either of these states exists, physicians have always been accustomed to believe that these must be determined principally by the symptoms which manifest themselves. Dr Brown, however, was of opinion, that the symptoms of diseases are not of themselves sufficient to enable a physician to form a correct judgment respecting their sthenic or asthenic character, or the greater or less degree of excitement that occurs in them ; and that, in forming such a judgment in particular instances, the practitioner should trust to his knowledge of the nature and action of the exciting powers by which the disease is produced, and of those by which it can be removed, there being in diseases, according to him, “not the least connection betwixt their appearance and real nature.” (Outlines, § 110.)

“Though excitement,” says he (Elements, § 71), “governs all the phenomena of life, yet the symptoms of disease which either its excess or deficiency produces, do not, of themselves, lead to any proper judgment respecting it ; on the contrary, their deceiving appearance has proved a source of infinite error.”—“Throughout this whole scale,” he observes (Elements, § 451), in speaking of his arrangement of sthenic diseases, “it is not so much the titles and names which have been used, but morbid energy which is regarded ; it being the certainty derived from the cause, not the uncertain and perfectly deceitful consideration of symptoms which was to be considered. The investigation of symptoms, which has hitherto been devoid of all benefit, has been of the highest detriment to the art (and as much, in medicine, the most productive source of fundamental blunders, as the question about abstract causes had been in

the other departments of philosophy), must be laid aside, and nosology strangled in its cradle."

In his recommendation to physicians to judge of the sthenic or asthenic character of diseases by the nature of the exciting powers which produce or remove them, and not by the symptoms which they exhibit (Elements, § 629), Dr Brown seems to have lost sight altogether of the generally acknowledged principle in medical philosophy, that all that can be known of the salutary or noxious powers of the external agents that act on the animal economy, must be derived from the observation of the sensible effects—whether healthy phenomena or morbid symptoms—which these agents produce. It is obvious, also, that he has everywhere, in his writings, confounded the attempts that have been made by practical physicians to judge of the presence of the states of vigour and debility by the symptoms which occur, particularly in the sanguiferous and nervous systems, with their endeavours to ascertain, and to express in short and accurate characters, the order which nature generally observes in the concourse and succession of symptoms that manifest themselves in the progress of individual diseases,—the two very distinct and important branches of medical science to which the names of Symptomatology and Nosology have been assigned.

How little even Dr Brown himself was able to dispense with the study and enumeration of symptoms, in characterising the states of Sthenia and Asthenia, may be easily judged of from the very copious detail which he has given in the fourth and fifth chapters of his Elements, entitled "The Sthenic and the Asthenic Diatheses, illustrated by an explanation of their symptoms," and

which, under the pretence of refuting and correcting the opinions of Dr Cullen, he has copied, in a great measure, if not altogether, from his lectures and writings. In these chapters, Dr Brown claims to himself the merit of having shewn that certain symptoms, which had been previously to his time considered as peculiar to an increased or to a diminished excitement, are liable to occur in both states of the constitution ; and, more particularly, that certain symptoms, which had been supposed to occur only when the constitution is in a state of increased excitement, are, in reality, more frequently the effects of debility, and can be removed only by stimulant remedies. The simple enumeration of the symptoms mentioned by Dr Brown as having been universally supposed by physicians to be always of a phlogistic nature, is sufficient to evince the futility of these pretensions. " Horror, sense of cold, and dryness of the skin," he remarks (Outlines, § 43), " frequency of pulse, paleness, headach and delirium, thirst and heat, hoarseness, cough and expectoration, inflammation, which have universally been supposed by physicians, in their diagnostic, pathognomonic, prognostic, and, last of all, in their nosological dissertations, to be always the same, and always such as were to be removed by an antiphlogistic plan of cure, are, however, in reality, as opposite affections as ever occur in the living system. Nay, so far are they from being always of a phlogistic nature, and to be removed by antiphlogistic regimen and remedies, that, on the contrary, they are, for the most part, of the asthenic kind, depending on debility, and to be removed by the most stimulating and invigorating powers."

That Dr Brown should have ventured to include in this enumeration, frequency of pulse, headach, and de-

lirium, in particular, shews how much he relied on the ignorance of those to whom he addressed himself, respecting the opinions of his predecessors. "We generally judge," remarks Dr Cullen, "of the force of the circulation by the state of the pulse. We suppose debility where the pulse is weak, and, by a necessary consequence, *frequent*, and also, in the same case, unavoidably irregular. I would say," he adds, "that in general—nay, very universally—the pulse is more frequent in nervous fevers than it is in any of the inflammatory, till the progress of the disease has induced that weakness which lays the foundation of the quick pulse."—"Headach," he observes, "is commonly considered as the first of the marks of increased impetus; but no morbid symptom is of more difficult theory than headach. Thus, I find that sometimes it appears with a slow pulse and pallid countenance; and there, certainly, it cannot depend upon an increased impetus: headach, therefore, is to be considered as an expression of the increased impetus, only when it occurs with more or fewer of the symptoms which I have mentioned. Headach is very often the first symptom in the attack of fever of the nervous kind; and it is nowhere more remarkable than through the course of this disease. But here the impetus of the blood does not appear to be considerably increased, so that I consider this kind of headach as a little uncertain."

Dr Brown's assertion (Elements, § 183, note *h*), that the fact of *delirium* sometimes depending on debility, and being in that case capable of being removed by stimulant remedies, "is as new, and of as much importance, as any fact in the whole work," contrasts in a remarkable manner with the candid declaration of Dr Cullen, contained in the following extract from his lectures on the Practice of Physic. In speaking of the use of wine in the treatment of

fevers, Dr Cullen observes, " I take my system, with regard to wine, from Sir John Pringle, and nobody has illustrated the use of it more agreeably to my notions. He lays the foundation in this manner : he observes that a delirium will arise from two different errors,—the one from large and repeated venesection, and the other from wine and other cordial medicines given in the beginning of the disease. Now, this is the foundation which I also would wish to lay. I have formerly pointed out these two cases of delirium, and, I hope, fully enough explained them ; the one depending upon a state of debility, the other upon a state of increased impetus, accompanied with the phlogistic diathesis ; and this explanation is fundamentally connected with the whole doctrine of fevers."

Dr Brown has dwelt at great length on the distinction between what he terms General Sthenic and General Asthenic Inflammation. In the sthenic, he conceived that the increased flow of blood into the vessels which are the seat of the inflammation, is owing to an increase in the tone of their fibres as living, and their density as simple solids ; whilst in the asthenic, it is owing to a greater atony and laxity in these vessels than in the rest of the vascular system. (Elements, § 207-8.) " General asthenic inflammation," he says (ibid. § 204-5), " is nothing else but asthenic diathesis, somewhat more violent in a part than in any other equal part. In other words, Inflammation, in this case, is nothing else but a state of the inflamed part, of the same kind with that of all the rest of the body, the inflammation being constituted by a lesser excitement in the part affected than in any other equal part." As examples of universal asthenic inflammation, he mentions that which accompanies



the gout, the putrid sore throat, the gangrenous sore throat, and that inflammation which produces sore eyes. "If, in the end of a typhus fever," says he (*ibid.* § 206), "inflammation affects the brain or its membranes,—which, though strongly asserted, is neither yet proved nor a very likely fact,—it will serve for an instance of an asthenic general inflammation." Dr Cullen was well aware that inflammation may occur in the progress of diseases, and particularly of fevers which exhibit marks of an extreme degree of debility. "Though inflammatory affections in any particular part," he remarks, "do commonly produce the diathesis over the whole economy, this is not universally the case: for we have innumerable instances of topical affections merely, which do not affect the sanguiferous system generally. It is," he observes, "an extremely dangerous state where this takes place with respect to the brain;" and accordingly in his lectures,\* he was at great pains in pointing out "the symptoms that express *irritation* applied to a certain part of the system, especially the brain, while there are other powers of debility acting in the same part, and preventing the effects of the irritation from appearing so considerable over the whole sanguiferous system."

Between those cases of acute diseases in which the symptoms of morbid vigour or debility are so mild as not to call for the interference of art, and those in which they are so distinct and so severe as to leave no room for doubt in the mind of the practitioner as to the course of treatment which he should pursue, there undoubtedly occurs in medical practice a variety of cases, in the progress of which it is a matter of the utmost difficulty to determine, from the symptoms that are

\* See Works, vol. i. p. 589.

present, whether stimulating or debilitating remedies should be had recourse to. Thus, for example, though the state of morbid excitement may, at an early stage of a febrile disease, appear to predominate, the practitioner may have learned from his observation of the general course of nature in similar diseases, or from his experience in the prevailing epidemic, that the opposite state, viz. that of debility, is liable to supervene in the progress of the disease; or, though the symptoms may be such as to indicate a great degree of debility in the general system, there may, at the same time, be marks of a determination of blood to some particular organ, the function of which is of immediate importance to life. It is unquestionably in the balancing of such opposite dangers as these,—in deciding on the relative importance of contradictory symptoms,—in the assistance derived in forming such a judgment from the consideration of collateral circumstances,—in watching the effects of the measures employed, and in withdrawing these, or even having recourse to measures of an opposite character when their use appears injurious, or at least to be no longer required,—that the skill of the experienced practitioner most conspicuously manifests itself. How often, in cases of this ambiguous character, notwithstanding all the consideration the practitioner is able to bestow upon them, does the failure of the measures which he employs leave him in a state of painful uncertainty, whether he ought to attribute the unsuccessful result to what Dr Cullen termed the irresistible tendency of the disease to a fatal issue, or to his having mistaken the measures best suited to obviate this event!

The principle laid down by Dr Brown, that the physician, after satisfying himself with regard to the nature and degree of the disease with which his patient is affected, should always "either stimulate or debilitate, and never desist from acting," is professedly founded on a disbelief in the existence of any power or powers in the animal economy which spontaneously tend to restore it, when affected with disease, to a healthy state. "At all times," as has been remarked by Dr Cullen, "physicians have observed that the animal economy has in itself a power or condition, by which, in many instances, it resists the injuries that threaten it, and by which it also, on many occasions, corrects or removes the disorders induced or arising in it. This power physicians very anciently attributed, under a vague idea, to an agent in the system, which they called Nature; and the language of a *vis conservatrix et medicatrix naturæ* has continued in the schools of medicine from the most ancient times to the present." Dr Brown entirely denies the existence of such a power in the economy. "If the healthy state," says he (Observ. on Spasm, § 28), "takes place because powers are applied, both proper in kind and in due proportion; while the diseased state is occasioned by the subduction of the external powers, or their ultimately excessive application; and all that without any visible interposition of any power in the constitution to alter their effect: lastly, if, neither in health, nor in predisposition, nor in the greatest number of diseases (for the *Vis Medicatrix* has been alleged chiefly, and almost only, to interpose in fevers), this imaginary power was ever once dreamed of; and if no sort of proof has ever been brought of its real existence in any case, what credit is due to the assumption of such a power?"—"To take this absurd supposition out of the way for ever," he continues (ibid. § 38), "we would ask what the powers are which have a tendency

to hurt and destroy the system, and which excite such motions as are suited to obviate the effects of the noxious power? The powers which, upon any occasion, operate upon us, have been enumerated, and are known; and the question is, Which of them are here meant for hurtful ones, which, in either excessive or deficient application, they may all be? I would ask a single instance of any one power operating with hurtful tendency, and, in the progress of that operation, converting its hurtful effect into a salutary one."

In contrasting the cures of diseases that are effected by nature with those which are accomplished by art, medical men have obviously meant, by the former term, to point out those cases in which diseases undergo a cure under the regulated use of the ordinary supporters of life, without the employment either of medicinal means or of surgical operations. In some instances, the state of disease is observed spontaneously to disappear, and that of health to return, without its being possible to discover, in any part of the economy, the occurrence of any phenomena by which the salutary changes can be accounted for. Examples of such salutary changes taking place in a slow and imperceptible manner, without the interference of art, are every day seen in the progress of febrile and inflammatory diseases, by the spontaneous return of the morbidly affected pulse and skin to their natural states, by the recurrence of appetite, and by the recovery of flesh and of strength. In other instances, again, the return to health is preceded by the occurrence of some obvious phenomena. Of this nature, may be considered the expulsion of noxious substances that have been introduced into the stomach,

either by vomiting or by diarrhoea ; the occurrence of spontaneous hæmorrhage in individuals whose vascular system is excessively full ; and the cessation from hæmorrhage from the occurrence of syncope in persons sustaining a great loss of blood ; as well as those changes which take place suddenly in fevers of an acute and severe character, in what has been denominated a Crisis, or solution of the disease, and by the occurrence of which a favourable stop is put to its progress, the patients frequently passing, in a few hours, from a state of great uneasiness and apparently extreme danger to one of comparative ease and safety. Of the same nature, too, may be regarded those local reparatory processes which take place in parts that have been divided, or otherwise injured by external violence ; the process by which hæmorrhage from a divided artery is arrested ; that by which blood that has been effused is gradually removed ; that by which divided parts, when brought into contact, are reunited ; that by which a new surface is formed on a part, the old surface of which has been removed by disease or by operation ; that by which the diffusion of pus is prevented in cases of suppuration ; that by which a new layer of bone is formed round a bone that has become dead ; that by which a dead bone, or any other foreign body, is expelled from an internal part ; that by which a dead portion is separated from the rest of the body, as a portion of the intestinal canal in consequence of invagination, or of a limb from mortification : these and other similar phenomena, depending on those nutritive and absorbent processes which seem to be constantly going on in the animal economy, both

in its healthy and in its morbid conditions, are said to be operations of the *Vis Medicatrix Naturæ*, because they are vital processes depending on the organic constitution of the animal economy, which the healing art, however much it may assist in promoting them, is unable to imitate.\*

But though the occurrence in the animal economy, when affected with diseases, of spontaneous changes calculated to restore it to a healthy state, had been universally admitted by medical men, till the time of Dr Brown, there has existed some difference in the opinions they have entertained respecting the manner in which these changes are produced; some having supposed, or at least having expressed themselves as if they supposed, that they take place in consequence of the active interference of an intelligent agent residing in and presiding over the economy, and bringing into operation the organs by which the changes necessary to effect a cure can be produced; whilst others have affirmed that we are to seek, in the primitive constitution of these organs themselves, in their state at the time, and in their mutual influences on one another, for the causes of those salutary changes. "Some," says Dr Cullen, "impute this power to the mechanism or organization of the body alone; others, such as the *Stahl*ians, impute it to the exertions of a rational soul, independent of the organization of the body, perceiving such things, as have a noxious tendency, and obviating their operation, in order to restore the body to a healthy state. Now, whether this power is to be imputed to the body or to the mind, it is not necessary to inquire here; the fact is enough, that there are symptoms which may be attributed to a *Vis*

\* See Appendix, Note O.

*Medicatrix Naturæ*, or, in other words, to a tendency in the economy to redress its own deviations."

In attributing to a particular power or agent, termed Nature, Autocrateia, *Vis Medicatrix Naturæ*, &c., those spontaneous changes occurring in the animal economy, which tend to restore it from a morbid to a healthy state, physicians, it may be allowed, have made the same inaccurate generalization, as they had done in ascribing the ordinary functions of the economy in the state of health to a power which they have denominated Soul, Sentient Faculty, *Archæus*, Vital Principle, &c. ; terms which, as has already been remarked, like that of excitability employed by Dr Brown, comprehend under them a variety of phenomena, which, in the present state of our knowledge, we ought to regard as truly referrible to the operation not of one, but of several different powers or vital properties. In both instances, probably, the error of physicians may be considered as having more frequently consisted in the employment of a general and indefinite expression, than in an erroneous view of the powers or functions with which the animal economy is endowed. In some cases, however, those who have made use of these terms, seem unquestionably to have lost sight of the fact that they are expressive not of a single specific property or power, but of the compound result of all the properties on which the exercise of the functions of the animal economy depends ; and have ascribed to what is merely a figurative personification of vital properties, the production of particular phenomena, real or supposed, of which the powers universally recognised as inherent in the economy, did not appear to

them to afford a satisfactory explanation. It is from the use of the same figurative language, that various other powers have been attributed to the economy which, in reality, are merely general expressions for the particular purpose effected by a number of different operations. Thus, when the animal economy is said to possess a power of assimilation, or a power of reproduction, it is obviously not meant to allege that these powers are simple and specific properties, in the same way as irritability and sensibility are ; but that certain parts of the economy are so constituted as to produce a particular class of vital phenomena or processes, which, as all conducing to a common end, it is convenient to designate by a common term.

The following quotation from Hoffmann, shews how little foundation Dr Brown had for affirming that the operation of the healing powers of nature had never been had recourse to by physicians, in explaining the phenomena of health, of predisposition, or of the greatest number of diseases ; but alleged chiefly, and almost only, to interpose in fevers. “ Of all the dietetic and pharmaceutical remedies which we employ,” says Hoffmann, (*De Medic. Simplic. & Opt.*), “ there is none which, by its own nature, absolutely possesses the power of protecting health or curing disease, but their virtues are only to be estimated in so far as they affect the nature of the human body. We have, therefore, not improperly said with Hippocrates, that nature cures diseases ; and chiefly brings about the action of remedies. But by the *nature of the human body*, which, according to Hippocrates, is the foundation and the beginning of all medical doctrine and demonstrations,—we understand nothing else, than the economy of the *vital* motions (progressive, combinatory, secretory, and excretory),



in the machine, merely hydraulic, or composed of various tubes, which are possessed of contractile and distractile powers. If these motions of the solids and fluids proceed rightly, and according to the laws of equality and temperament, then all the functions which the human body exercises, are carried on according to rule; and not only life, but the integrity of body and soul, is preserved. To watch these motions attentively, and when they deviate from proportion or symmetry, when they become too strong, or when, from various causes, they slacken or become deficient, then to reduce them to order and moderation by proper remedies, that is rightly judged to be the only and principal business of the physician." From these observations of Hoffmann, it is evident that it was not any peculiar property different from those concerned in the performance of the ordinary functions of the economy, that physicians have had in view in speaking of the *Vis Mediatricis Naturæ*; but only the concurrence of a number of vital processes in the production of a common effect,—that effect being the restoration of the economy from a morbid to a healthy condition.

If the circumstance of its proceeding from his own pen could give additional weight to the refutation of any of Dr Brown's opinions, it may be affirmed that, in the following passage, the existence of the healing powers of Nature is as fully and amply recognised, as had ever been done by Dr Cullen. "Through this whole division of local affections," says Dr Brown, speaking of those 'where no effect but in the hurt part arises,' "there is a certain *energy of nature*, which tends to the restoration of the healthy state; but it is not the celebrated *Vis Mediatricis Naturæ* of physicians; for in this case nothing else happens, but what equally happens in the cure of general diseases. If proper remedies are applied, the sound state in

both sorts of diseases, follows. If the remedies be neglected, the solution of continuity degenerates into a worse and worse nature, and then into gangrene, or the death of the part. It is the excitability, or that property of life by which the functions are produced, that, wherever life, whether in a part, or over the whole body, is hurt, procures the return of the healthy state, by means of the external powers acting upon it. It is, then, the excitability, affected by the action of those powers, that is to say, the excitement, which governs the state of the solids, both in parts, and over the whole body." (Elem. § 701.)

Dr Brown's denial of the existence of the healing powers of nature,—understanding by that term the spontaneous occurrence in the animal economy, when threatened or affected with disease, of vital changes calculated to counteract the injurious operation of external agents, and to restore the functions to a healthy state when any morbid deviation has occurred in them,—seems to have had the same origin to which several other of his opinions have been traced, an exaggerated perversion of generalizations, some of a speculative, and others of a more practical nature, which had been previously formed and expressed by Dr Cullen in his lectures and writings. The Stahlian doctrine of the existence, in the economy, of an intelligent agent, which, perceiving the tendency of noxious powers threatening, or of disorders anywise arising in the system, immediately excites such motions in the body, as are suited to obviate the hurtful or pernicious consequences that might otherwise take place,—appeared to Dr Cullen to be calculated to lead physicians into, or to continue them in a weak and feeble practice, and, at the same time, to supersede or discourage all the attempts of art. Al-

though, therefore, he contends that the operation, in diseases, of the *Vis Medicatrix Naturæ*, or, in other words, of the vital mechanism of the animal economy, must unavoidably be admitted as a fact, yet he somewhat incautiously, and perhaps even inconsistently with his general views, alleges that “wherever this principle is admitted, it throws an obscurity upon our system; and that it is only where the impotence of our art is very manifest and considerable, that we ought to admit of it in practice.” (See Works, vol. i. p. 406-7.) And accordingly, in Dr Cullen’s writings, allusion is more frequently made to the influence of the healing powers of Nature, as producing some of the phenomena of diseases, than as superseding the necessity of medical treatment.

It would be unjust to imagine that physicians, in ascribing conservative and healing powers to the animal economy, or in asserting that such is its constitution, had meant to allege that in all the circumstances in which noxious agents operate upon this economy, motions are excited in it sufficient for producing their expulsion, or for obviating their effects. Those who have most strenuously inculcated the propriety of the practitioner availing himself of, and trusting to the powers or efforts of nature, as they have been termed, which tend to the restoration of health, have at the same time acknowledged that, in some instances, Nature seems to make no efforts whatsoever to resist the action of morbid causes; that in others the efforts which she makes, are insufficient to produce this effect, and require the assistance of art; and that in other instances, again, her efforts are injurious, and require to be counteract-

ed. Dr Brown, in his recommendation of a constant and universal activity in the practice of the medical art, seems entirely to have overlooked the distinction that exists between *those* diseases which are known to disappear without the employment of remedies, or the cure of which has generally been attributed to Nature ; —*those* which can only be removed by the employment of remedies, or in which nature has been said to require the assistance of art ;—and *those* which prove fatal, however skilfully remedies may have been employed. In few things, however, do the superior knowledge and skill of the experienced practitioner shew themselves more evidently than in the accuracy with which he distinguishes between these three classes of morbid affections ; the caution and prudence which he observes in interfering with the natural course of diseases, and the decision, promptitude, and efficacy of his measures, in the circumstances in which they may be useful. What a contrast to this does the ignorant and presumptuous practitioner exhibit, who, in the treatment of his patients, is perpetually meddling with and thwarting the salutary operations of Nature ; employing himself busily, when he ought to be a simple spectator, and remaining confused or idle during the occurrence of morbid phenomena that call for the most prompt and vigorous interference of art.

In conformity with the general principles, physiological, pathological, and therapeutical, set forth in the foregoing propositions, Dr Brown undertook to expound the nature, history, and treatment of particular diseases ; and he has, both in his avowed and unavowed writings, lost no opportunity of asserting the

immense superiority of his own modes of practice, as compared with those which had been followed by preceding physicians. Notwithstanding the length to which our examination of Dr Brown's claims to original improvement in the *theory* of medicine has extended, it seems proper, before concluding the review of his system, to institute a slight comparison of the plans of treatment recommended by him to be pursued in some of the principal diseases to which the economy is subject, with those which Dr Cullen had advised for the same diseases. This comparison, while it will shew that not a few of Dr Brown's practical statements and doctrines had their origin in a desire to contradict precepts inculcated by Dr Cullen in his lectures and writings, will, at the same time, evince how wide a difference there is between universal and unqualified canons, founded entirely upon speculative views of a practical art, and restricted, qualified, and prudent rules, framed in conformity with the general experience of medical practitioners, and corrected or improved by that of their judicious and reflecting suggester. As might be anticipated from his general view of the comparatively greater frequency of asthenic than of sthenic diseases, the methods of cure enjoined in particular diseases by Dr Brown will be found to differ from those recommended by Dr Cullen and other experienced physicians, chiefly in the more extended application of the stimulant plan of treatment as respects both regimen and medicines.

There is, perhaps, no part of the writings of Dr Cullen calculated to convey a higher impression of his sagacity as a practical observer, or of his perspicuity as

a teacher, than the account which he has given of the circumstances under which debilitating and stimulating remedies, and, in particular, bloodletting and the use of wine and opium, should be had recourse to, in the treatment of fevers. With respect to the treatment of Continued Fevers, his general principle was, that the choice of remedies must be guided by the character of the symptoms which may appear to prevail in the particular case, whether these shall be of an inflammatory, of a nervous, or of a putrescent or bilious character. From the frequent combination of the inflammatory diathesis with the febrile state, the antiphlogistic plan of treatment appeared to Dr Cullen to be of very frequent applicability in the treatment of Continued Fevers (First Lines, § 127.); but, at the same time, he was at great pains to point out the circumstances which indicate the employment of stimulant remedies (ibid. § 201), and particularly of wine (ibid. § 219), which he considered as the safest form of stimulant in this class of diseases. (See Works, vol. i. p.644.) Dr Brown, on the other hand, maintained that antiphlogistic remedies are seldom if ever required in any form of fever, whether in that which had been termed Synocha (or inflammatory fever) or in Typhus (or low fever); and that, in the latter form of fever in particular, the hopes of the practitioner must be placed entirely in the free use of stimulant remedies. "Synocha," he informs us, "is one of those mild sthenic diseases in which bleeding is not necessary, and bleeding with any degree of freedom is hurtful." (Elements, § 482.) Of those more gentle sthenic diseases, amongst which he includes Synocha, he says that, "while it is very easy to distinguish them from the asthenic diseases resembling them, if, how-

ever, any person should think the marks of distinction ambiguous, let him know that, upon account of that gentleness, though the disease under examination should be sthenic, blood is not even to be let, much less when there is the apprehension that it may turn out asthenic, to which last so debilitating a power is destructive; and with that information," says he, "let him understand that his method of cure, conducted in that way, will be fenced and secured from all mistake. For if the diathesis, though sthenic, be slight, bleeding will often precipitate it into the opposite, and will at least be useless. If, on the contrary, the disease which passes for a sthenic one, should, in its progress, shew itself an evident asthenic one, in that case, every drop of blood which may have been taken, will go to the increase of the disease. Yet this pernicious and daily practice sends more men out of this world, than all the curses of human life." § 490.

It may safely be left to the intelligent practitioner to determine whether these precepts of Dr Brown's, regarding the practice of bloodletting in Inflammatory Fever, or those expressed by Dr Cullen in the passages that have been referred to, are most consonant with the results of experience; or most likely to secure the practitioner from all hazard of mistake, and the patient from the well-known consequences of inflammatory reaction.

One of Dr Brown's most arrogant claims in regard to the Practice of Physic, is that of having been the first to explain the proper principles on which the administration of Opium should be conducted. "Opium," says he (Elements, § 232, note *h.*), "though much used in the cure of certain symptoms of diseases, was never understood by those physicians who, in books and lectures, assumed to themselves the province of directing the profession

of physic. Every property they assigned to it was the reverse of the truth." In support of this somewhat startling allegation, he affirms, 1<sup>st</sup>, that physicians, instead of allowing opium to be the strongest stimulant in nature, made it a sedative; 2<sup>d</sup>, that they ascribed to it the property of bringing on sleep, whereas, it is the most powerful body of all others in producing and keeping up the watching state; and 3<sup>d</sup>, he insinuates that, in attributing to it the power of allaying pain, they were not aware that it increases the pain accompanying inflammation, and, besides, that it aggravates every other symptom of the disease.

It would be unjust to Dr Cullen to pass over these assertions without examination, the therapeutical operation of opium having been, as we shall presently see, a subject on which he had bestowed much consideration; and such an examination is the more necessary, that Dr Brown's pretensions to original views respecting the administration of opium have been recognised by some of his commentators. Thus, Dr Beddoes alleges that, "in pamphlets recommending repeated doses of opium to support excitement, it would be easy to detect attempts to purloin Brown's language and ideas;" and M. Rouillier says, that "we owe great obligations to Brown for the manner in which he has developed his ideas on the tonic and stimulant property of opium."

As to the nature of the operation (stimulant or sedative) exerted by opium on the economy, Dr Cullen had remarked, in speaking of this substance, in the lectures on *Materia Medica* delivered by him in 1761 (when Brown first became his pupil), and surreptitiously



published, as we have seen, ten years afterwards, that “Not only have the ancients disputed whether this medicinal substance was hot or cold, but we are at this moment disputing whether it acts chiefly as a stimulant, or whether in any case it acts directly as a sedative (*Vide* Tralles on Opium). These doubts probably arise from the effects of opium being mixed, and proving different according to the dose, the time of exhibition, and the state of the patient.” And after describing the succession of phenomena which follow the administration of opium, Dr Cullen observes,—“It is easy to see that, from the stimulant power of opium, it may be an excellent cordial, though, at the same time, when the stimulant power is great, or other stimuli subsist in the system, it will produce fever and inflammation. On the contrary, by its sedative power, it may be even used to correct motion, except in the heart and vessels, that is, in fever.”

In prosecuting farther the principles on which it may be understood in what manner opium is hurtful or salutary in different diseases, Dr Cullen, in these lectures, considered, *1st*, its use as an anodyne ; *2d*, its use in fevers, and *3d*, its use in increased evacuations.

With regard to the anodyne qualities of opium, on which Dr Brown's third pretension to discovery is founded, Dr Cullen observes that “Pain may be considered as of three kinds, arising from three different causes, viz. Distention, Spasm, and Irritation ; from Distention, more especially of the inflammatory kind, as in the pleurisy ; from Spasm, more frequently in the alimentary canal, as in colic ; from Irritation, or acrid stimulants applied, as in cancer.” With regard to the use of opium in Pain from inflammatory distention, he remarks that “As opium, in its first operation, increases the circulation and impetus of the blood, and even, in its last operation, accumulates the

blood in the larger vessels, and thus brings a stimulus to the heart, it must increase inflammatory distention. From all this," he continues, "it is easily understood, why opium is hurtful in inflammatory pains. But there are some inflammatory diseases which, though founded on an inflammatory diathesis, are apt to become chronic, as the rheumatism. In chronic rheumatism, in the absence of fever, in pain of long standing and confined to a particular part, opium may be employed for a temporary relief; but, in no one instance, does it contribute to the cure; nay, even in pains of the longest standing, and confined to the most single part, it will often be hurtful by increasing the irritation, which gives us great caution as to the free use of it in such cases."—

"To the head of inflammatory distention, also, must be referred the use of opium in the Gout. The pains occurring in the extremities, in this disease, are certainly of the inflammatory kind, and, therefore, opium may be supposed to increase these, which, indeed, I have seen it do, though in so far it is safe, as the health of the body depends on this inflammation. Whether we ought to proceed farther and take off the pain, has been much disputed. The sounder practitioners, as Sydenham, are justly of opinion that the more violent the pain, the shorter is its duration, and the less hurtful to the system. If the pain be so violent as to overcome all patience, they admit that opium may be given; but then it is very justly added, that it is always with danger of the disease attacking other parts more violently; and I myself have seen instances of this. The rule, then, is this, that opium ought not to be employed at the first attack of the gout, nor even at the height of the disease, unless some violent symptom ensue from mere irritation of pain. When the pains are gone, it may be given; but then, as weakening the tone of the stomach, it debilitates that power upon which depends the healthy termination of the gout. However, I must confess, that, at the end of the disease, I have often seen good effects follow from it, restoring the patient

soon to health, and preventing many an uneasy night. I have known also some who obtained the same effects from taking to a course of spirituous liquors immediately after the removal of the pains. I imagine, then, that, if the stimulant effect of the opium take place here without much of the sedative, and at the same time as sweat is promoted, the opium will act chiefly as a strengthener." (4th edition, p. 335.)

With regard to the pains arising from Spasms, Dr Cullen observes, that these are most effectually in all cases cured by opium. "At all times," he continues, "there have been disputes about the virtues of extraordinary medicines. While Hecquet, at Paris, carries the use of opium to an extravagant length, on the other hand, the Stahlans will not admit it at all, saying it operates merely as a palliative without removing the cause. Most manifestly here it has a different effect, not only removing the sense of pain, but also its cause." There can be little if any doubt, that this last observation of Dr Cullen's was the source of the following remark by Dr Brown: "The truth is, it (opium) is not a palliator of pain, but a remover of its cause, as often as that depends upon debility, while it as certainly aggravates every other kind of pain."

It will not, I presume, be necessary to discuss Dr Brown's other claim to discovery in regard to the operation of opium on the economy, that, namely, of its *preventing* sleep; but, before leaving the subject, I think it necessary to take a slight review of the doctrines taught by Dr Cullen, at different times, respecting the employment of opium in fever, in vindication of a statement made by himself in his treatise on *Materia Medica*, published in 1789, which contains the only

reference to Dr Brown that is to be found in any of his writings. "Many," he there says, "are fond of a more free use of opium in continued fever than had been common before, and have believed that this was introduced by a certain noted teacher and author; but I assert, that I myself was the first who freely and largely employed opium in fevers, under certain restrictions, indeed, which, neglected by other practitioners, have occasioned much mischief."

In his lectures on *Materia Medica*, in 1761, Dr Cullen, in speaking of the use of opium in fever, after observing that, "in intermittent fevers, the use of opium was common and constant among the ancients, and we have in this endeavoured to imitate them in modern times," proceeded to observe; "In continued fevers, opium has been more frequently employed. Tralles has bestowed much pains on this part of his subject; but he appears to me to argue like a man who had already settled his point, and to have been prejudiced against it" [the use of opium in continued fevers], "nay, indeed, to overlook, or rather to be totally ignorant of, the considerations which should determine the question. In inflammatory fevers, opium is certainly hurtful; but all fevers are not of this kind, either in their beginning or continuance, and everybody allows now the existence of the nervous fever, or that wherein the *vis vitæ* is apt to sink. In the case of the nervous fever, opium may be used as a stimulant; and where the remissions, and consequently the accessions, are distinct, as a sedative. In the last case, it may be used in the same manner as the bark, and whenever a distinct accession comes on and it is useful to throw in the bark, opium may be also employed. Of these things, Tralles takes no notice. Wine, I think, is an analogous remedy to opium, being both sedative and stimulant. In the last intention, it is preferable to opium, as it can be given in a smaller dose, and also, from the acid which accompanies it, it is less inflammatory, and therefore, on this

account, in doubtful cases, it may be more effectually used, as well as from the subdivided and gradual manner in which we can exhibit it. But I have seen fevers attended with very strong spasmodic affections, where camphire, musk, &c. were used, where the opium was of much more consequence, and even, as I have seen, it removed delirium itself; and, indeed, I believe it may be said universally, there is no case in which we use wine where we may not also employ opium." (P. 344-5.)

Dr D. Campbell of Lancaster, in his *Observations on the Typhus, or Low Contagious Fever, &c.* published in 1785, refers to Dr Cullen as having, in his lectures on the Practice of Physic in the year 1769 (*i. e.* in his first course on that branch), stated, that opium is particularly useful in every case of delirium that arises from irritation; and as contending, too, that there is a species of delirium apt to occur in the course of continued fever, which, wanting the characteristics of the phrenitic or inflammatory state, arises from irritation, and is only to be cured by large doses of opium.

The following is the more extended view of this subject, which is contained in his manuscript lectures on the Practice of Physic, as delivered some years afterwards.

"It seems to be universally established in the judgment of physicians, that opium is hurtful in all inflammatory diseases, and therefore is not at all admissible in the pure inflammatory fever; nay, that, from the same consideration, it may be of doubtful or hazardous use in the beginning of most fevers, wherever there is any suspicion of a phlogistic diathesis being present. It is very probably from a neglect of this, that we meet with so many complaints among practitioners as to the mischievous effects of opium in fevers.

The principal writer upon opium is Dr Tralles, who has given us two 4to volumes upon the subject. He has been at great pains to render it universal, that opium is extremely hurtful in fevers; but I am not much influenced by his opinion, and if it was fit to enter into a particular criticism, I could shew that he forms most of his conclusions from a few instances of its abuse in the cases I speak of, where the disease was manifestly inflammatory. I must, however, say, that it ought to be attended to that opium may be hurtful by its stimulant power, and *that* especially in every case of inflammation or tendency to it, where there is any degree of phlogistic diathesis. But I have shewn that there are cases of fever directly opposite to this, in which stimulants, such as cold and bark, may be necessary, and have the most useful effects; and in all such cases of fever, I have no doubt that opium may be employed for its stimulus, as wine generally is. In inflammatory fevers, opium is certainly hurtful; but all fevers are not of this kind, either in their beginning or their continuance, and everybody allows now the existence of nervous fever, or that wherein the *vis vitæ* is apt to sink. In the case of nervous fever, opium may be used as a *stimulant*.

“ I say, therefore, that there are many circumstances in which the stimulant power of opium may be useful in fevers; but it probably may be more useful as a sedative power, and we know well that it is so in all cases of *irritation* that are not inflammatory. In all cases of pain and irritation proceeding from any thing but the stimulus of topical inflammation,—and especially when it proceeds from spasm,—the sedative power of opium is the most powerful means of obviating it that the practice of medicine is yet acquainted with.

“ I find that opium especially takes off all irritations powerfully affecting the nervous system, with certainty and safety. The exhibition of opium is commonly, in the first instance, stimulant to the heart and arteries; but when the dose is

carried far enough, it produces a slow pulse, as occurs in other cases of a comatose state. From the power of opium in taking off irritations, especially in the nervous system, there are, no doubt, many cases of fever in which its sedative power may be advantageously employed, though we may not be able to ascertain these cases with exactness.

“I will give one pretty clear instance. We know certainly that the brain may be in a state of considerable excitement from other causes than merely the increased impetus of the blood. This we know from many species of mania, from the length of time they subsist, the coldness of the body, the slowness of the pulse, and their transitory nature; and I think I am very certain that such a maniacal delirium, without any inflammatory state, or other cause of increased impetus of the blood in the brain, does frequently occur in the advanced state of fevers; and in several instances of this kind, I have seen the disease cured, and the delirium quieted, by large doses of opium, and, indeed, in these cases, by large doses only. Such an appearance of a maniacal delirium, attended with some impetuosity and furor, occurring in fevers, is generally imputed to a phrenitic state, to more or less of inflammation in the brain; and it is to be acknowledged that it very often is justly so imputed. But I have seen several instances of the impetuous or furious delirium, as to which, from their suddenly admitting of a solution, and from their frequently being cured by the use of opium, I conclude for certain, whatever the appearances may have been, that they were not truly of the inflammatory kind. I own that though, in general, I can distinguish between the mild and furious delirium, and am pretty confident that it is proper in the furious to reject, and in the mild to admit of, opium, there is an uncertainty that attends the whole of this affair. Sir John Pringle marks out the phrenitic delirium when the eye becomes wild and the voice quick; but he does not go further, leaving us to this general view; and there he very properly avoids wine, and would

certainly avoid opiates too. But I have seen that wild look and quick voice, with a languid and weak pulse, and with a peculiar redness of the eyes, which were what I would call decisive marks of inflammation ; and yet I have seen these cases pass away into a state of tranquillity, and manifestly cured by inflammatory medicines, by wine, and by opium. It appears, therefore, that there are such cases as will admit of these remedies ; and I must leave you, in the course of practice, to find out other means of being more exact in this matter.\*

“ I pass on to a more general case, in which the sedative power of opium may be more useful, where, indeed, all inflammatory diathesis is very manifestly gone, or perhaps never strongly subsisted, and that spasm which depends upon debility only remains. Here, I imagine, the sedative power of opium may be employed more frequently than it is. The combination of its stimulant, joined to its sedative power, may be of use ; for if, by the sedative power, we can take off the spasm of the extreme vessels, we may, by the stimulant power, excite to a considerable degree the heart and arteries more effectually to procure sweat ; and we do this more safely by opium than by any other stimulant power whatever, not combined with such a sedative.

“ We have been, for these hundred years past, or thereby, in the humour of blaming the age before us, the age of alexipharmics, when the stimulating and heating medicines were employed, and their principal stimulus was opium. Now, I cannot believe that the practitioners in these days were quite so blind as to go on in this practice if they had found it universally hurtful ; and I must conclude that, in many cases, they found it safe and successful, so that there are more cases where opium might be employed, than might at first sight be supposed. I cannot, however, go back

\* It may, perhaps, be questioned, whether the cases here referred to by Dr Cullen were not, in reality, cases of delirium tremens—a disease which has come to be understood only since his time.



to proper enough facts, and therefore must take them as they are given us in later times. A very remarkable one to our present purpose is what we find in Dr Lind of Haslar's appendix to his work on the Diseases of Europeans in Warm Climates. From frequent experience, he has found that opium may be employed in the hot fit of intermittents; that it brings on more equally the sweats that are to give the solution, and gives a more complete apyrexia than is to be obtained by any other means, and he seems to recommend it as, in general, a useful practice."

For the cure of intermittent fevers, Dr Cullen had pointed out two general plans of treatment,—the employment of stimulant, and the employment of tonic, remedies. The stimulant plan of treatment he regarded as neither very safe nor very efficacious; and he consequently gave a decided preference to the use of tonics, and particularly of bark. (Works, vol. i. p. 672.) Dr Brown adopted opinions respecting the treatment of intermittents directly the reverse of those of Dr Cullen. He affirmed, that it is from the use of stimulant remedies only that we can hope to derive benefit in the treatment of intermittent fevers; and he represented bark as a remedy nearly, if not altogether, inefficacious; alleging, that "before the Peruvian, and some other barks of similar operation, were found out to act as remedies, a variety of strong drinks were used with sufficient success;" and that "it now also is found and demonstrated, in fact, that the diffusible stimuli are by far more effectual than any bark, and that the bark often fails, while they are perfectly effectual in the re-establishment of health." (§ 655.) In speaking of the failure of bark in the cure of intermittent fevers, he asks (note *d.* § 666), "If that medicine,

with its universal high character, shall turn out next to an imposition, what are we to think of testimonies in favour of any thing? I have most generally," he adds, "found an analogy betwixt the remedies that are, in reality, powerful, and our ordinary supporters of health. The wines and strong drinks are certainly a part of diet with most people, and so is opium among the Turks. But what analogy can be found betwixt the same ordinary supports of life, the same durable and natural stimuli, and the bark of a tree whether brought from South America, or growing among ourselves? I will not pretend to say, that the bark is devoid of all virtue; but I must have greater proof of its power over disease than I have yet met with, before I can retract much of what I have said."

Of the diseases referred by Dr Cullen to the order Phlegmasiæ, and considered by him as requiring an antiphlogistic treatment, a portion only, as we have already seen, were admitted by Dr Brown to be general or constitutional diseases, the rest being declared by him to be only local affections. The treatment which he lays down for the diseases which he admits to be constitutional phlegmasiæ, does not differ from that recommended by Dr Cullen, except in the reluctance with which he recommends the employment of evacuant remedies, and particularly of bloodletting. In the treatment of what he considered as local phlegmasiæ, however,—as Gastritis, Enteritis, Hysteritis, &c.—the employment of general evacuant remedies is proscribed; and in the last mentioned disease, the employment even of stimulant remedies strongly recommended. Thus, with regard to the treatment of gastritis, he states (Elements, § 707), that "as this is a local disease, the indications of diminishing increased, or increasing diminished excitement over all, do not apply. On the con-

trary, unless a general disease happen to be combined with it, nothing else is to be done but by throwing in bland demulcent liquors, to defend the tender part from the rude contact of the stomach's contents, and give the inflammation time to finish its course ; and if the physician is called soon enough, to wash off the hurtful matter with a diluent drink." He farther informs us, that "when gangrene is seated in the alimentary canal, the method of cure is to pour in spirits and laudanum ; when the shut viscera are affected, to place some hope in the same and other stimuli, but much less. And as the same remedies also suit gangrene when it is external, consequently liquid laudanum should be rubbed in upon the dying part ; spirits should be poured upon it ; the parts already dead should be cut out ; the edge of the living parts all round should be stimulated, and an inflammation made in it. In sphacelus, the remedies are, in general, the same as in gangrene ; but they should be stronger, and administered in greater quantity and with greater nicety, and in less expectation of a cure." (§ 747-8.)

Respecting inflammation of the uterus, or hysteritis, Dr Cullen, in his lectures on the Practice of Physic, had said (Works, ii. 83), that "it frequently lays the foundation of child-bed fever ; but these fevers are not constantly of the inflammatory kind,—they are often nervous, and perhaps miliary, neither of which admit of venesection ; and nothing but necessity will prescribe venesection for lying-in women, when we consider the evacuations they have already suffered. If the pain, however, depends on an inflammatory affection of the uterus, nothing but venesection affords relief," &c. (§ 422.)

Dr Brown tells us, that, "in inflammation of the womb, bleeding, according to the common practice, and any mode of evacuation, are not to be practised, nor is the patient to be forbid to eat ; but, in the first place, regard is to be had to

the affected part ; the body must be laid in a horizontal posture ; she must be kept from motion, and be allowed rich soups and wine. By-and-bye, more solid animal food should be used, morsel by morsel, but frequently repeated ; and if the debility should get ahead, recourse must be had to more wine, drink still stronger, and opiates,—the use of which last should not be neglected even at first." (§ 723.) It need scarcely be remarked, that the progress of knowledge has confirmed Dr Cullen's view of the different characters which puerperal fever may assume, and, consequently, of the different modes of treatment which it may require. But it has brought no confirmation of the plan of treatment recommended by Dr Brown to be pursued in cases of inflammation of the womb.

The treatment of Gout necessarily holds an important place in a review of Dr Brown's system of Practical Medicine ; for he frequently informs his reader that it was the circumstance of his attention having been directed to the treatment of this disease in his own person, which ultimately led him to a knowledge of the imperfections of the medical doctrines then subsisting in the schools, and to the discovery of the more correct principles which he taught.

Dr Cullen, in his view of Gout, recognised two forms, viz. the Regular and the Misplaced, in which the principal source of danger consists in the predominance of inflammatory affection : and two forms, also,—viz. the Atonic and the Retrocedent,—in which the danger chiefly depends on the occurrence of spasm in some of the muscular cavities. In the two forms of inflammatory gout, he conceived that the plan of treat-

ment must be strictly antiphlogistic ; whilst, in the two forms of spasmodic gout, the treatment, he conceived, should consist in the employment of tonic and of stimulant remedies. In speaking of ardent spirits as a remedy in the retrocedent gout, Dr Cullen observes; " It is remarkable that the dose must be very large. I have known an instance of a person taking two pounds of brandy or strong rum to produce a cure. In this case, persons then bear it with impunity, who would have been thrown into a temporary fever by the fourth part of the quantity taken at another time. I need not speak of some analogies of various affections of the nervous system which will bear unusual and uncommonly large doses of opium, such as tetanus ; and, perhaps, that is to be attended to in other cases. It appears from hence, that there is something in the system that resists the usual effects of opium, or any such narcotic ; and a similar circumstance occurs in the case of spirits employed in the gout ; for whilst, in ordinary health, four ounces of spirits would have given some degree of intoxication, and double that quantity would have effectually intoxicated them, persons in this fit will take double that quantity without any appearance of intoxication ; and till the pain is removed out of the stomach, they find no such symptoms. But as soon as it has overcome the violence of the spasm, an intoxication is immediately induced, and a stupor comes on ; and, in many cases, we would be puzzled how to measure the doses, were it not for that circumstance ; but so long as we find that it does not take any effect upon the nervous system, in producing more or less of the symptoms of intoxication, we are still safe in pushing the dose farther."

Dr Brown, in admitting a gout of stronger and a gout of weaker persons, seems to have had in view a distinction similar to that of Cullen's into inflammatory and spasmodic gout. But in both forms of the

affection, he maintains that it is by the employment of stimulant remedies only, that the disease can be removed.

“The gout of stronger persons,” says he (§ 608), “is not a disease of strength, or a sthenic one. It does not depend upon vigour of the constitution and plethora, as has been commonly hitherto imagined, but is manifestly asthenic. It is not to be treated by an antisthenic, as has hitherto been the notion, but by a sthenic plan of cure. To prolong the intervals of health and prevent a fit, the remedies are rich food, taken in plenty, of the animal kind, with a rejection of all sorts of vegetable matter or a very sparing use of it, strong drink, a full quantity of blood, no evacuations,” &c. —“In the gout of weaker persons, and certain diseases of the alimentary canal which are of a common nature with it (colicodynia, dyspepsodynia, and the violent hysteria), the cure of every one of them must be stimulant. When each of them is but slight, beef-soup, and similar rich ones, &c.; and afterwards, solid animal food and moderately diluted drink. In a higher degree of violence of any of them, while the soups should still be continued, at same time pure strong drinks should be administered. And when the violence of any case baffles this whole form of stimulus, recourse must be had to musk, volatile alkali, camphor, ether, and opium. These must be administered in large doses.”

“The diffusible stimuli are so powerful in removing the inflammation of the gout, that sometimes strong drink undiluted, as wine and spirits, or the latter diluted with water as warm as can be borne, have, in a few hours, removed the most violent fit, and restored the use of the affected foot. And the same remedies are of equal efficacy in removing the general symptoms. Treated in the way here mentioned, the most violent degree of the disease always gave way in a few days, and milder cases in as many hours. I never found a single case baffle me but one,” &c. (§ 213.)

Dr Brown’s practical recommendations with regard

to the diseases comprehended by Dr Cullen under his class Neuroses, afford some as glaring blunders as those which have been already pointed out in the Pyrexiaë.

Where an attack of Apoplexy has taken place, Dr Cullen conceived that, in all cases of a full habit, and where the disease has been preceded by marks of a plethoric state, bloodletting ought to be immediately employed, and very largely; and its operation aided by purging and blistering. For the prevention of the disease, he conceived that where the predisposition to apoplexy manifests itself in early life, a low diet, with a good deal of exercise, will probably prevent the disease. At the same time, he suggests that it might not be safe to put upon a low diet persons who are advanced in life before they think of taking precautions, and who are, at the same time, of a corpulent habit, which generally supposes their having been accustomed to full living; but remarks, that it may be sufficient to render their diet more moderate than usual, especially with respect to animal food.

But according to Dr Brown, apoplexy arises chiefly from indirect debility, that is from the excessive use of stimulants; and as each kind of debility is increased by the other, he concludes that it is improper, in apoplexy, to have recourse to evacuant remedies. "The effect of the debilitating plan of cure," says he, "is so pernicious in apoplexy, that it is received as a rule, that the third fit is not often, the fourth never, got the better of." (Elements, § 638.)—"To prevent the fits of apoplexy, all excessive stimulus must be avoided, in such a manner that the body may be invigorated, and direct debility guarded against. The stimulant plan of cure should be set

on foot with moderation and accuracy ; and in the place of the forms of stimuli which have, either from long or excessive use, lost their stimulant operation, others, which the excitability, not yet worn out with respect to them, can receive, should be substituted ; that is, the kinds of food, of drink, and of diffusible stimuli, should be changed all round, and upon the failure of each lately used one, those which have been long ago laid aside should be resumed." (§ 640.)

It appeared to Dr Cullen that the treatment of Palsy must be conducted very much in the same way as that of apoplexy ; and particularly that, in hemiplegia, internal stimulants are almost always equally dangerous as in the cases of complete apoplexy. Even where the disease has subsisted for some time, and the apoplectic symptoms,—or those marking a considerable compression of the origin of the nerves,—are removed, he conceived that the use of stimulants is an ambiguous remedy. Dr Brown, on the other hand, affirms that debilitating and evacuant remedies are to be avoided in palsy, and that the principal remedies are those which have the greatest power in invigorating the surface of the body ; whilst none of the powers endowed with stimulant virtue by any means should be omitted. We should, therefore, he says, employ a great deal of an opiate, the influence of which upon the surface is the most considerable of all other powers. (Elements, § 635.)

Dr Cullen, in considering a plethoric state of the system to be one of the predisponent causes of Epilepsy, conceived that this state is to be corrected chiefly by a proper management of exercise and diet. " With respect to the latter," he remarks, " it is particularly to be observed here, that an abstemious course has been frequently found to



be the most certain means of curing epilepsy. I have had several instances myself," he added in his lectures, "both in young and in older persons, where a very great degree of abstinence entirely cured the disease, or rendered its return far less frequent than it was before; and when the abstinence was not rigidly enough observed, or other causes of fulness or turgescence happened to be applied, it has again returned." (Works, vol. ii. p. 430.) At the same time he was aware, that epilepsy may be occasioned by debility, and when this has proceeded from inanition, he observes, the strength may be restored by restoring the fulness and tension of the vessels by a nourishing diet (p. 431); and we have had instances of the propriety and success of such a practice." Dr Brown's view of epilepsy made no provision for any such distinctions. "For the purpose of preventing epilepsy," he says, "we must avoid debilitating powers. The vessels should be filled by giving food as nourishing and as effectual in producing blood as possible, &c. The strength must be fortified by the Peruvian bark, if the approach of the fits can be perceived, and by wine, and the more diffusible stimuli." (§ 630.) "The same remedies which radically cure the gout, also cure epilepsy, and precisely in the same manner." (§ 631.)

Dr Cullen had observed, that, "From the frequent mixture of dyspepsia with Hysteria, it has been common to prescribe to our patients a full animal diet, than which nothing has been more pernicious. On the contrary, a spare diet has been of remarkable service in preventing the recurrence of the disease." (Works, vol. ii. p. 503.) Dr Brown's notions of the proper method of treating hysteria are widely different. "The fits of gentle hysteria," says he, "are soon removed by small dozes of opium, repeated at short intervals. The intervals should be secured from danger by full diet, and a moderate and natural stimulant management," (§ 583.) "The treatment of the vio-

lent hysteria corresponds with that of the gout of weakened persons." Of a similar import, and not less indicative of his thorough ignorance of the treatment of diseases, are his recommendations relative to Hypochondriasis, "in the cure of which," he tells us, "the stimulus of food, drink, and others, should be employed. The patient should be kept cheerful, &c. He should have generous wine given him, to relieve the symptoms of the stomach and intestines, and to raise his animal spirits. And if these should fail of success, the diffusible stimuli, as opiates, should have their turn for a time, for the purpose of striking a stroke at once." (§ 619.)

As a counterpart to the plan of treatment recommended by Dr Brown for hypochondriasis, we may quote his method of cure in Mania. "The patient," says he, "should be struck with fear and terror, and driven, in his state of insanity, to despair. As a remedy against the great excitement of the organs of voluntary motion, the labour of draft cattle should be imposed upon him, and assiduously continued. The diet should be the poorest possible, and his drink only water. In water, as cold as possible, the patient should be immersed, and kept under it, covered all over, for a long time, till he is near killed." (§ 496.)

The account which Dr Brown gives of Thirst, which he seems to raise from the rank of a symptom to that of a disease, may not only amuse the reader, but illustrate and corroborate an observation formerly hinted at, viz. that a large portion of Dr Brown's notions of medical practice was derived from the management of drunkards and gluttons, that portion of his system which relates to the treatment of the diseases of indirect debility being obviously founded on the vulgar maxim of being cured by a hair of the dog that bit you.

“ There are two causes,” Dr Brown tells us, “ of as many affections which have only got one name between them, that of *Thirst*, the one is sthenic, the other asthenic. The former arises from the stimulus of salt, of rich and plentiful meals, of heat and labour, and some others ; never ending in vomiting till the sthenic state is over, which is seldom. The asthenic thirst depends upon pure debility, sometimes indirect, sometimes direct. Its tendency is always to stomach sickness, and, as that increases, to vomiting.” (§ 557-8.) —“ The thirst which is occasioned by debility, is increased by draughts of cold water ; is hurried on to nausea and vomiting ; is quenched by pure wine or spirit, which prevent the troublesome symptoms that would otherwise follow. Pure wine increases the thirst which proceeds from a sthenic cause, and excites the same troublesome symptoms which cold water does in the other case ; cold water allays it, and prevents the future tumult.” (§ 109.)

The asthenic form of thirst seems to have been a disease to which Dr Brown had had occasion to pay much attention, and, accordingly, he gives us very full information respecting its history and its appropriate treatment.

“ There is, “ he says, “ a very frequently occurring affection, beginning with thirst, and proceeding to vomiting. It often proceeds no farther than those symptoms ; it oftener ushers in the most severe affections, such as sometimes dyspepsia or indigestion, sometimes colic, sometimes the gout sometimes proper fevers, and many other asthenic diseases.” —“ In a slighter degree of asthenic thirst, a glass or two of brandy, or of any similar spirit, or, which is a better rule, given till the complaint is removed, is sufficient. It should be either pure, or diluted with a very little hot water. That should be followed by eating some animal food,” &c.—“ When the thirst, not quenched by these means, proceeds in its farther stages, viz. vomiting, excruciating pain, diar-

rhœa, and colic without pain, in all those cases, recourse must be had to a larger dose of the drink ; and when that does not succeed to our wish, we must next fly to opium, and other more diffusible stimuli, if they are to be found. When, by these, relief is procured, rich and pure soups, without fat, should, from time to time, be poured in, and the canal be carefully bathed all over with them." (§ 556, 562, 565.)

Such are a few specimens of the practical knowledge of diseases and their treatment possessed by the man whom, on the strength of his own arrogant pretensions and virulent detractions and vituperations of others, some persons of great learning and ability have allowed themselves to regard as the greatest reformer of medical science and practice whom the world ever saw !

Of the many misconceptions that have, at different times and in different places, been entertained respecting the qualifications of Dr Brown to be the reformer of the science of medicine, there is certainly none more extraordinary, and none more wide from the truth, than the supposition of his having been led to his peculiar opinions by the results of his personal intercourse with the sick. It is doubtful whether up to the time of the publication of his *Elements* he had ever had a patient under his charge. Indeed, it was well known in Edinburgh, that it was only in the latter period of his life, after he had come forth as the inventor of a new system, that Dr Brown gave himself out for, or was ever employed as, a medical practitioner.

In a chapter entitled " Of Dr Brown's private practice," Dr Beddoes makes the following observations, p. \*ciii. " To some readers it may appear strange that I

should have finished the life of a physician, who caused so great an uproar in the medical world, without more particular notice of his private practice. I inquired with some solicitude, but in vain, whether, during the long period of his studies, he was peculiarly observant of diseases." — "Brown somewhere condescends to speak of his own 'very large practice;' but this is a compliment which every medical writer apparently thinks that usage justifies him in paying to himself. I remember to have heard of a house at Edinburgh, which the Brunonians opened for the reception of poor patients; probably they were not able to procure funds for its permanent support. The founder of the sect was, I believe, seldom consulted but in cases given up as hopeless, and he was then apt to speak with imprudent confidence."

The success of his practice was, as well as its extent, a subject on which Dr Brown was in the habit of paying himself compliments, which, whatever effect they might produce on the ignorant and credulous, were ill-calculated to inspire confidence or belief in the minds of people of intelligence and medical experience. "Here, then, at once," says he, "is a discovery, upon scientific principles, of the true nature and certain cure, of more than one-half the diseases of the human race; the method of cure arising from this doctrine having never failed in any of them, and having never succeeded upon a contrary plan, the debilitant and evacuant, so universally recommended by the authority of the schools." (Elements, § 194, note *s*. See also § 196, note *c*.; § 213, note *i*.; § 243, note *a*. &c.)

Dr Brown, indeed, does not seem to have very decidedly determined in his own mind whether or not his system, besides furnishing to mankind an almost universal panacea, in the form of the stimulant plan of cure, had also supplied them with the elixir of life.

For though he states at one place "that if the just degree of excitement could be constantly kept up, mankind would enjoy eternal good health" (Elements, § 70.); at another place, he acknowledges that "though a most exact measure of existence be kept up, yet death, at last, however late, supervenes." (Elements, § 29.)

Besides personal experience, a reformer in medicine might be expected to be well versed in the opinions of those who had preceded him in the same path of inquiry. Dr Parr has remarked that only one author, Triller, is quoted in the whole of the *Elementa*, and that even with him Brown does not seem to have been intimately acquainted; and the very attentive perusal of his writings has not furnished me with any reasons for believing that his knowledge of medical literature extended much beyond the information contained in the lectures of his teachers, and particularly in those of Dr Cullen.

As an illustration of Dr Brown's ignorance of medical literature, as well as of the intrepidity with which he was wont to put forth his own claims to originality, we may refer to his assertion that, in medical writings, previous to his time, "no idea seems ever to have been formed of the body as a whole," and that "it was never dreamed that there was one overruling principle throughout, upon which all the functions depended." (Elements, § 232, note *e*.) The well-known comparison of the body to a circle, instituted by Hippocrates, which might be supposed to be familiar to every tyro in medicine, affords in itself an ample refutation of this statement. "To me," says

the Father of Physic, "there seems to be no beginning in the body, but all things are at once beginning and end. For one inscribing a circle finds no beginning." In more recent times, Dr Brown might have found the same idea expressed with considerable apophthegmatic force by Dr Pitcairn, who has said that "all the viscera are created together, and at the same time; nor is there any one which is older than another, for all effects become in their turn causes of their causes." And Boerhaave, as if conjoining in one proposition the statements of Hippocrates and Pitcairn, remarks, "all parts of the body are so connected with one another, that, running as it were into an orb, they stand reciprocally in the relations of cause and effect."

Nor do the writings of Dr Brown's own preceptors in medicine shew less distinctly the absurdity of his pretensions in this respect. Dr Whytt, as we have already seen, recognised in the most distinct manner, as one of the forms of corporeal sympathy, that which is "general, and extended through the whole system, every sensible part of the body having a sympathy with the whole." Dr Cullen also had remarked in his lectures on *Materia Medica* in 1761, which were attended by Brown, "that the functions of the animal economy are mutually so much the causes and effects of each other, and run so much in a circle, that it is difficult to choose where to begin" (4to edit. p. 3); and Dr Brown must often have heard him in his lectures on Physiology, express himself to the following effect: "Every view we can take of the animal economy, leads us to consider it as a system, as a whole consisting of parts, each of which contributes to the subsisting of the

whole.”—“ The animal economy is, I say, a system consisting of parts mutually necessary to one another, and to the subsistence of the whole ; but I shall have occasion to say also, that there is some difference in the importance of these parts, that it is pretty evident that the most important part to the economy is the brain. One word more upon the subject, and it is this, that the connection and dependence of all the parts of the system appears to be constantly necessary from what we commonly observe.”

As a farther illustration of Dr Brown's gross ignorance of medical literature, it may be mentioned that though he makes frequent mention of Stahl, and claims to himself the merit of having given the first complete refutation of his system, he invariably misspells his name ; styles him Professor of Medicine and Chemistry in Berlin ; and alleges that he “ did not give his works, either medical or chemical, in writing, himself, but attested copies of both done by Juncker.” (Observations on Spasm, § 164 and 171, note.)

Whether Dr Brown was actually the dupe of his own ingenuity, or secretly laughed at the credulity of those who received his vague speculations as the philosophical inductions of “ a sure and cautious observer of the phenomena of nature,” I cannot pretend to determine. There can be little doubt, however, that these speculations had their origin in personal spite arising out of wounded vanity ; and the malignant and rancorous animosity displayed in the writings in which they are expounded, take away the pleasure which might have been derived from the manifestation of talent such as he has evinced, even in the support of erroneous opinions.

Few authors seem to have better comprehended the



spirit in which Dr Brown's opposition to Dr Cullen's pathological and therapeutical doctrines was conceived, or to have more justly appreciated the estimation to which it was entitled, than Professor Scuderi of Modena, who, in his *Introduzione alla Storia della Medicina Antica e Moderna*, published in 1794, observes :

“ Brown, in the second part of his *Compend*, pretends to refute the system of Cullen ; and certainly, if some feeble objections, joined to violent abuse and the most marked hatred and bitterness against the reputation of a celebrated man, merit the name of refutation, no one has succeeded better than Brown in that difficult undertaking. But in place of a calm examination of the fundamental principles established by the illustrious Reformer of medicine, and a rational criticism of the physiological system of medicine, Brown confines himself to an attack on the doctrine of Spasm, as he calls it, and on the *Vis Medicatrix Naturæ*, as if Cullen had given spasm for the sole proximate cause of every disease, and the *Vis Medicatrix Naturæ* for a universal law. Is spasm the sole corner-stone of the doctrine of Cullen ? is it placed in front as the source of every morbid affection to which the human body is subject ? It is, it may be said, represented by him as the cause of fever : But does Cullen, in reality, make the proximate and primitive cause of fever consist in spasm, or not rather in the diminished energy of the brain, followed and accompanied by spasm of the skin ? Let the reader examine with attention the text of Cullen, and decide how far the criticism of Brown is applicable. With regard to the *Vis Medicatrix Naturæ*, those who have studied the works of the Scotch Boerhaave know in what estimation he himself held it ; what objections he urges to the views of the Stahlians on this subject ; and in how judicious and correct a point of view he considers this law of the animal economy in his excellent treatise on Fever. What, then, is the reputation of Brown ? A perpetual invective against the

superior merit of his illustrious colleague [teacher]. And what difference is there between the two systems? That which distinguishes a great and salutary reform from a pernicious and unsatisfactory hypothesis."

Though the doctrines of Dr Brown, which we have been considering, were, at their first promulgation, embraced by some of the younger and more ardent medical students in Edinburgh, and by a few practitioners in different parts of Great Britain, they were never generally adopted by the medical profession in this country, nor regarded by them as either sound in principle or safe in practice. In some of the medical schools on the continent of Europe, however, and particularly in those of Italy, these doctrines appear to have been enthusiastically received by several of the teachers, and by large numbers of the students; and in different parts of Germany they were adopted by various pathologists of high reputation. In pretending to explain all the phenomena, healthy and morbid, of the animal economy, by the single and universal principle of Excitability, or Life, Dr Brown's system delivered its followers from the drudgery necessarily attendant on acquiring a knowledge of the numerous and complicated doctrines of the simple solids and humoral pathology, at that time not completely banished from the medical schools of these countries; while it expressed its leading dogmas in a language so entirely new, as to communicate to those who adopted it the distinguishing badge of a sect, and to instil into their minds the flattering idea that, in embracing it, they were become the reformers, rather than the mere students of the science they were

cultivating. Such a theory of medicine could not fail to find followers among those who had grown weary of poring over the old and stale doctrines of the medical schools ; and who,—participating in the innovating spirit then beginning to prevail in Europe, in the natural and metaphysical sciences, as well as in the political,—were in search of opinions apparently more in unison than these doctrines with the advancing state of medicine, and of its collateral sciences. That the system of Dr Brown did not obtain a reception equally favourable in France as it met with in Italy and Germany, may, it is conceived, in some measure be accounted for by the attention of the medical students of that country having, at the time of its appearance, been begun to be occupied with the Vital Doctrines of Bordeu and Barthez, in the school of Montpellier, and in part also, perhaps, by the jealous reluctance which many of the medical men of that nation have shown to adopt opinions originating in England.

It was in northern Italy, and particularly in the medical schools of Pavia, Pisa, Padua, Parma, Milan, Turin, and Bologna, that the Brunonian doctrines attracted the largest share of attention, and were subjected to the most elaborate and rigorous discussions. It may be questioned whether the enthusiastic reception which these doctrines at first met with in that country, was not in part attributable to the very limited knowledge which most of its physicians had acquired of the pathological views of Hoffmann ; and of the extension and improvements which Dr Cullen, in availing himself of the experimental labours of Haller, Whytt, and others, had given to the pathology of

the nervous system. And even the views which Baglivi had opened up, relative to the importance of the state of the solids in the production of the morbid phenomena of the animal economy, and the share which, in later times, some Italian physiologists had taken in prosecuting the inquiry into the distinct nature of irritability and sensibility, do not appear to have exerted any very considerable influence on the pathological opinions entertained at this period by their countrymen.

Among the teachers of practical medicine in northern Italy contemporary with Dr Cullen, Borsieri, John Peter Frank, and Vacca Berlinghieri, may be considered as having justly enjoyed the highest reputation. Neither Borsieri nor Frank appear from their writings to have had much acquaintance with the works of Dr Cullen; and the severe, and, in many respects, unjust criticism of Dr Cullen's *First Lines*, published by Vacca in 1787,<sup>1</sup> was calculated to lower the Edinburgh Professor in the estimation of Italian pathologists, and, of consequence, to disincline them to the study of his writings.

The doctrines of Brown were first introduced to the knowledge of medical men in Italy by Drs Moscati, Rasori, and Joseph, son of John Peter, Frank.<sup>2</sup> The first of these physicians published an edition of the *Elementa Medicinæ* at Milan in 1792, with a preface of his

<sup>1</sup> *Saggio intorno alle principali e piu frequenti Malattie del Corpo Umano, e de' Rimedj piu valorosi di esse.*

<sup>2</sup> Dr Brown's son mentions (*Life of his Father*, p. 163), that an Italian translation of the *Elementa* was published by Vincent Solenghi, but does not say when or where. This translation I have not seen.

own, principally with the view, as he states, of exciting doubts respecting the soundness of the humoral doctrines of pathology, which still continued to prevail in Italy. In the same year Rasori translated into the Italian language Dr Brown's Compend or Outlines of the New Doctrine, which he accompanied with a preliminary discourse, and with notes in illustration and support of the Brunonian system. In the preface to this work, Rasori largely insists on the great benefits that must result to all departments of medical science from the application of the system of Brown. In 1795, Dr Joseph Frank published at Pavia a translation into Italian of the Inquiry into the State of Medicine, which had been brought out in Dr R. Jones's name (See p. 223 of this vol.); and in 1796, he published a translation into the same language from the German, of a General View of the Theoretical and Practical Doctrines of Brown, which had been published the year before at Frankfort by Dr Weikard. By none of these three writers were any doubts expressed as to the originality, validity, or practical importance of Dr Brown's opinions; and from the medical writings of the period, these doctrines appear to have been very warmly embraced by the students, and extensively carried into practical effect by many Italian physicians.

The history of Brunonianism in Italy, subsequently to its being introduced to the knowledge of the physicians of that country by the above mentioned editions and translations of Dr Brown's works, might perhaps be advantageously divided into three periods; the first extending to the occurrence of a Petechial epidemic

fever in Genoa, during the siege of that city by the English and Austrians, in 1799–1800; or rather, to the publication of Dr Rasori's account of this fever in 1801;<sup>1</sup> the second period extending to the publication, by Professor Tommasini of Bologna, in 1817, of an Introductory Lecture or *Prolusion*, as it is termed, to his course on Clinical Medicine,<sup>2</sup> in which he endeavoured to incorporate the physiological, pathological, and therapeutical speculations of himself and his countrymen in one general system, under the designation of the “New Italian Medical Doctrine;” and the third period, reaching from the appearance of that prolusion to the present time.

During the first of these periods (from 1792 to 1801), the system of Brown was subjected to a very full and able critical discussion by several Italian physicians, in essays and treatises professedly directed to this object, as by Dr Sacchi,<sup>3</sup> assisted, it has been supposed, by his teacher, Professor Carminati of Pavia; by Strambio;<sup>4</sup> by Vacca-Berlinghieri;<sup>5</sup> by Menegazzi;<sup>6</sup> and by Antonini.<sup>7</sup> It was commented on also

<sup>1</sup> Storia della Febbre Petecchiale di Genova, &c. Milano, ix. (1800-1.)

<sup>2</sup> Della Nuova Dottrina Medica Italiana, Prolusione alle Lezioni di Clinica Medica nella Pontif. Univers. di Bologna. 1816-17.

<sup>3</sup> In Principia Theoriæ Brunonianæ Animadversiones. Papiæ, 1793. (Biblioth. Browniana, i. 1797.)

<sup>4</sup> Riflessioni sul libro intitolato Joan. Brunonis Elementa Medicinæ. Milano, 1795. (Biblioth. Brown. iv.)

<sup>5</sup> Meditazioni sull'Uomo Malatto, e sulla Nuova Dottrina Medica di Brown. 1796.

<sup>6</sup> Animadversiones Medicæ in Doctrinam J. Brunonis. 1800.

<sup>7</sup> Osservazioni sul Sistema dell' Eccitabilita Browniana, Ve-

by Professor Scuderi, of Modena;<sup>1</sup> and particular points of its doctrines were discussed in dissertations by Marzari, of Treviso;<sup>2</sup> and Professor Michelotti,<sup>3</sup> of Turin. Cattaneo<sup>4</sup> seems to have been the only author who, during this period, undertook the defence of the Brunonian system; whilst an attempt at reconciling the contending views then existing in medical science, was made by an anonymous author in a work entitled *Preliminaries of a Peace*,<sup>5</sup> &c., published at Pavia in 1799, but bearing in the introduction the date of 1797, and professing to be a translation from the German, though without any direct proof of its having previously appeared in that language.

During the second period (from 1801 to 1817), the literature of Brunonianism is of a more varied nature than during the first. Several works were published, professedly discussing its doctrines, as by an anonymous author at Milan,<sup>6</sup> and by Professor Canaveri of Turin.<sup>7</sup> At the commencement of this period, also, Tommasini published a very instructive course of

nezia, 1800; and reprinted in "*Il Brownianismo Discusso*," Milano, Anno ix. (1800-1), with a reply by Dr J. L.

<sup>1</sup> *Introduzione alla Storia della Medicina.* 1794.

<sup>2</sup> In *Giorn. per servire, &c.*, vol. ix. 1793-4.

<sup>3</sup> In *Giorn. per servire, &c.* 1796.

<sup>4</sup> *Memorie intorno quei punti delle opere de' Signori Vacca e Scuderi che riguardano il Sistema di Brown.* Giorn. Med.-Chir. Milano, Ottobre 1795.

<sup>5</sup> *Preliminari di una pace Medica ossia alcuni punti di riunione fra Brown ed i suoi Avversari.* Pavia, 1799.

<sup>6</sup> *Confutazione del Sistema di Brown.* Milan, Anno x. (1801-2).

<sup>7</sup> *Analyse et Refutation du Systeme de Brown.* 1804.

lectures on Physiology and Pathology,<sup>1</sup> which he had delivered some years previously, and in which he supported several of the Brunonian doctrines, and endeavoured to defend them from the attacks that had been made upon them by some of his countrymen. But the works which appeared during this period, that exercised the most important influence on the progress of medical opinion in Italy, were not professedly devoted either to the confutation or to the defence of the Brunonian system, but directed chiefly to the discussion of various points in theoretical and practical medicine. Among these may be mentioned, as more particularly deserving of notice, besides Rasori's account of the Petechial Fever of Genoa, which has been already named as marking the commencement of this period, Guani's reflections on the same malady, which he designates as the Epidemic of Liguria;<sup>2</sup> Dissertations by Tommasini<sup>3</sup> and Professor Rubini of Parma,<sup>4</sup> on Yellow Fever, occasioned by the alleged occurrence of that disease at Leghorn in 1804; and the systematic work on Fevers by Giannini;<sup>5</sup> as well as the Pathological Essays of Ambri on the Transmutation of Dia-

<sup>1</sup> *Lezioni Critiche di Fisiologia e di Patologia.* Parma, 1802-4. (Republished in the *Raccolta di Opere Mediche Moderne Italiane.* Bologna, 1827, tom. 1-4).

<sup>2</sup> *Riflessioni sull' Epidemia della Liguria, ossia Saggio di una Nuova Teoria sulle Malattie Epidemiche e Contagiose.* Genova, Anno v. (1801.)

<sup>3</sup> *Ricerche Patologiche sulla Febbre di Livorno del 1804, sulla Febbre Gialla Americana, &c.* Parma, 1805.

<sup>4</sup> *Riflessioni s. Febbric hiamate Gialle, e su i Contagi in genere,* n. *Giorn. d. Soc. Med. Chir. di Parma*, vol. viii. 1805.

<sup>5</sup> *Della Natura d. Febbri e del miglior metodo di curarle.* Milano, 1805.



thesis;<sup>1</sup> of Bondioli on the method to be followed in Experiments on the *Materia Medica*,<sup>2</sup> on the Particular Forms of Universal Diseases,<sup>3</sup> and on Irritative Action;<sup>4</sup> of Testa on Organic Actions and Reactions;<sup>5</sup> of Fanzago on the Essential Differences of Universal Diseases,<sup>6</sup> and on Irritative Action;<sup>7</sup> of Bufalini on the Doctrines of Life and of Irritation;<sup>8</sup> of Geromini on the Pathogeny of Dropsies;<sup>9</sup> and of Rubini on the theory of Irritation:<sup>10</sup> to which fall to be added various Essays, published during this period, by Rasori<sup>11</sup> and

<sup>1</sup> Memoria sulla Trasmutazione delle Diatesi, n. Giorn. d. Soc. Med. Chir. di Parma. 1801.

<sup>2</sup> Sopra l'Esperienza ed il metodo da seguirsi nelle Ricerche di *Materia Medica*. 1803. (v. Dissertazioni raccolte dal Dott. Monti. Bologna, 1826, p. 17.)

<sup>3</sup> Ricerche sopra le Forme Particolari delle Malattie Universalì. 1805. (v. Dissert. p. 41.)

<sup>4</sup> Ricerche sull' Azione Irritativa, 1808. (v. Dissert. p. 101.)

<sup>5</sup> Delle Azioni e Reazioni Organiche, Crema. 1807.

<sup>6</sup> Saggio sulle Differenze Essenziali delle Malattie Universalì. Padova, 1809.

<sup>7</sup> Sull' Azione Irritativa, n. Giorn., di Medicina Practica di Brera, i. 327. 1811.

<sup>8</sup> Saggio sulla Dottrina della Vita. Forlì. 1813. (Republished in the Raccolta, &c., tom. v. 122.) Annotazioni sul Tifo e sulla dottrina dell' Irritazione, n. Giornale di Brera, x. 1817.

<sup>9</sup> Saggio sulla Genesi dell' Idrope. Cremona, 1816.

<sup>10</sup> Storia di una Dyspnœa consensuale, con alcune riflessioni sulla Teoria dell' Irritazione. Parma, 1812.

<sup>11</sup> Dell' Azione della Digitale Purpurea sull' Economia Animale, 1811. (v. Opuscoli di Medicina Clinica. Napoli, 1832, p. 63. Raccolta, viii. 104.)

Dell' uso della Gommagutte nei Flussi Intestinali e del Nitro nel Diabete. (v. Opuscoli, p. 111. Raccolta, vii. 22.)

Delle Peripneumonie Infiammatorie, e del curarle, principalmente col Tartaro Stibiato. Milano, 1810. (v. Opuscoli, p. 152. Originally published in the Annali di Scienze e Lettere, vol. vii. Milano, 1811.)

Tommasini,<sup>1</sup> on the Action and Use of certain remedies generally regarded as Sedative.

The third period of Brunonianism in Italy commences with the publication of the Prolusion of Tommasini, already noticed, in which the views that had been propounded by the Italian pathologists, during the previous periods, and particularly those of Rasori and of Tommasini himself, are set forth under the designation of the New Italian Doctrine of Medicine. The correctness of the principles of this doctrine was subjected to an elaborate discussion, soon after the publication of the Prolusion, by Spallanzani,<sup>2</sup> nephew of the celebrated naturalist, and a few years afterwards, by Geromini.<sup>3</sup>

Subsequently to the publication of Tommasini's Prolusion, the event which seems to have given the greatest impulse to the new Italian medical doctrine, was the issuing, first in 1819, and again in 1821, by the Society of Sciences resident at Modena, of a theme for a prize-essay, in the following terms. "To determine whether the ideas that are given in the modern schools of medicine, of Excitability and of Excitement, are sufficiently accu-

<sup>1</sup> Sugli effetti della Digitale Purpurea di Linneo. Mem. letta alla Soc. Med. Chir. di Parma al 1<sup>o</sup> Dec. 1806. (Dissert. ed Altri Scritti. t. iii. 5. Bologna, 1823.)

Sull' azione deprimente o Contro-stimolante di alcuni rimedj. Mem. letta, &c., al 1<sup>o</sup> Maggio, 1809. (Ibid. iii. 73.)

Ricerche sull' azione de Vescicanti e de' Rubefacenti ed esame della cosi detta azione Rivellente. Nel Giorn della Soc. Med. Chir. de Parma, ii. 1806. (Ibid. iv. 197.)

<sup>2</sup> Lettere Critiche sulla Nuova Dottrina Med. Italiana. Reggio, 1818.

<sup>3</sup> Saggio di un Anal. dei Fondamenti dell' Odierna Dottrina Medica Italiana, n. Annali di Omodei, 1822. (Vol. xxi. and xxii.)

rate and precise; and, in case they be not so, to determine what variations ought to be made in respect of either of them; and from thence to deduce what are the precise ideas that ought to be formed of Diathesis, both hypersthenic and hyposthenic, of Irritation, of Stimuli, of Contro-stimuli, and of Irritating powers." None of the essays given in on the first of these occasions seem to have been considered as entitled to the first prize, but an *accessit* was adjudged to that of Guani;<sup>1</sup> on the second occasion, this theme elicited no fewer than ten essays; and of these there were printed those of Emiliani<sup>2</sup> and of Bufalini,<sup>3</sup> to which the first prize and an *accessit* were awarded. The discussion of the comparative merits of these various essays, and of the doctrines contained in them, called forth a number of additional treatises, particularly from the pens of Bergonzi,<sup>4</sup> Geromini,<sup>5</sup> Bufalini,<sup>6</sup> &c.

During the same period there were published a variety of essays and treatises on the different pathological and therapeutical questions involved in the so-called New Medical Doctrine. Among these may be particularly noticed that of Penolazzi on the Theory of Irritation;<sup>7</sup> that of Guani on Contro-stimulus, and

<sup>1</sup> Mem. in risposta al quesito propos. dalla Societ. Ital. Modena, 1821.

<sup>2</sup> Risposta al Tema proposto dalla Soc. Ital. di Scienze residente in Modena, &c. Firenze, 1824.

<sup>3</sup> Intorno al Tema proposto dalla Soc. Italiana, &c. Firenze, 1824.

<sup>4</sup> Confronto Crit. d. Memorie di Emiliani e di Bufalini. Parma, 1824.

<sup>5</sup> Dottrina Medica Bufaliniana Compendiata e Discussa. 8vo. Milan, 1826.

<sup>6</sup> Intorno alla Medicina Analitica Cicalate. Milan, 1825.

<sup>7</sup> Saggio sulla Teoria dell' Irritazione. Padova, 1817.

Irritative Diseases ;<sup>1</sup> those of Amoretti on the Vital Principle ;<sup>2</sup> that of Rolando on the different species of Excitability and Excitement, &c. ;<sup>3</sup> those of Rasori,<sup>4</sup> Emiliani,<sup>5</sup> Puccinotti,<sup>6</sup> and Tommasini<sup>7</sup> on Inflammation ; and those of Pistelli on the Nature of Inflammation, and on Diathesis.<sup>8</sup> Nor could a summary of the medical literature of Italy during this period be satisfactory, that did not include the more systematic elementary outlines of pathological science, in which the medical literature of no country is more, if equally rich ; such as those of Gallini,<sup>9</sup> Acerbi,<sup>10</sup> Fanzago,<sup>11</sup> Bufalini,<sup>12</sup> Vulpes,<sup>13</sup> Dalla Decima,<sup>14</sup> &c.

<sup>1</sup> Del Contro-stimolo, e delle Malattie Irritative. n. Comment. Med. di Brera, 1819. (v. Raccolta, tom. v. 77.)

<sup>2</sup> Nuova Dottrina Medica della Vitalità e dello Stimolo. Torino, 1818.

Discuss. Med. Filos. sul Principio e sulla Potenza o Forza Vitale. Torino, 1823.

<sup>3</sup> Cenni Fisio-patologiche sulle differenti specie d' Eccitabilità ed Eccitamenti, sull' Irritabilità, e sulle Potenze Eccitanti, Debitanti ed Irritative, &c. 1821.

<sup>4</sup> Teoria della Flogosi.

<sup>5</sup> Della Infiammazione Commentario. Modena, 1824.

<sup>6</sup> Del Processo Flogistico, e di alcune altre proprietà della Flogosi, 8vo. Roma, 1821.

<sup>7</sup> Della Infiammazione, e della Febbre Continua, Considerazioni Patologico Pratiche. Pisa, 1820. 3d ediz. Bologna, 1827.

<sup>8</sup> Sulla Natura della Infiammazione Ric. Patolog, &c., Annali Univ. di Medicina, t. xi. p. 5. Rifless. Crit. sulla così detta Diatesi dei Medici Moderni, &c. &c., negli annali cit. 1821, t. xx. p. 5.

<sup>9</sup> Saggio di Proposizioni Elementari di Patologia.

<sup>10</sup> Annotazioni di Medicina Pratica. Milano, 1819.

<sup>11</sup> Istituzione Patologiche, 1813 ; sec. ediz. Livorno, 1824.

<sup>12</sup> Fondamenti di Patologia Analitica. Pavia, 1819.

3d ediz., 1828-1830.

<sup>13</sup> Istituzioni di Patologia Generale. 2da ediz. Napoli, 1830.

<sup>14</sup> Istituzioni di Patologia Generale. Padova, 1819.

Of the questions arising out of the examination of the Brunonian doctrines that have chiefly engaged the attention of Italian pathologists, and been subjected by them to animated and intelligent discussion, the following may be enumerated as among the principal :—

*1st*, Whether there exists in the animal economy such a property as was recognised by Brown under the name of Excitability ; if so, whether it should be regarded as of a single or manifold nature ; what are its characters ; whether it be liable to waste, and capable of repair ? &c.

*2d*, Whether the Fluids be primarily liable to morbid or pathological conditions ; and, if so, what influence these conditions exercise in producing the various diseases of the economy ?

*3d*, Whether all the powers which act on the economy exercise, as Brown alleged, one common mode of operation, viz., Stimulation ; or whether they operate variously ?—and in connection with this inquiry the doctrine of the existence of a class of substances which operate, directly, in a manner the reverse of that of stimulation, viz., Contro-stimulants ; and the question, whether particular substances act directly on particular parts of the economy—such as the heart, the lungs, the bowels, the kidneys, &c., so as to modify the manner in which they execute their functions ?

*4th*, On what foundations the doctrine of Diatheses rests ; and the propriety of the substitution, by Rasi, of the Diatheses of Stimulus and Contro-stimu-

lus, in place of those of Sthenia and Asthenia, which Brown had proposed ?

5th, Whether there exists a general morbid state of the economy different from the diatheses of sthenia and asthenia, or of stimulus and contro-stimulus, such as Guani proposed to designate by the term Irritation ; and whether there exist powers or agents which operate on the economy by inducing that state, or by removing it ?

6th, Whether a disease can undergo a change in respect of its diathesis, from sthenia to asthenia, or *vice versa*, during its progress ?

7th, Whether the opposite diatheses of sthenia and asthenia may coexist in different parts of the economy ?

8th, What are the proper grounds of distinction between Universal and Local diseases ; and, particularly, whether the circumstance of being attended or unattended by diathesis constitutes this distinction ?

9th, Whether there exist other elements of the variety which diseases exhibit besides degrees of sthenic or asthenic diathesis ; and, particularly, what importance is to be attached to the *Forms* of diseases, as marked by their nosological characters ; and to their *Pathological Conditions*, or the visible or invisible morbid alterations of the solids and fluids which produce them ?

10th, What are the grounds of distinction between Organic and Dynamic diseases ; and whether there must not exist, in all diseases, some change in the organic constitution of the subject ?

11th, Whether there exists such a state as Indirect debility ?

12th, Whether Inflammation be ever of an asthenic character?

13th, Whether Fever be, in all instances, symptomatic of inflammation? And lastly,

14th, Whether the quantity of particular stimulant or contro-stimulant remedies which the economy is capable of bearing, or its Tolerance of remedies, is regulated by its particular morbid conditions, and the degrees of these?

A short review of some of these leading inquiries, and of the doctrines relative to them, propounded or maintained in the writings of the Italian pathologists, may serve to shew not only the ingenuity, acuteness, and intelligence with which they have been investigated, but likewise the relation that these inquiries, and the present scheme of pathological science adopted in Italy, bear to the doctrines of Cullen and of Brown respectively. The author is apprehensive, that, in taking this review, it will scarcely be possible for him to avoid some repetition of what has already been said, from the circumstance that the greater part of the critical examination of Brown's doctrines was written before he had acquired any distinct knowledge of the Italian investigations.

1. The constant theme of commendation among the Italian followers of Brown, has been the wonderful simplicity which he had introduced into the study of the animal economy, by the recognition of the single power of Excitability. But these pathologists seem to have, in a great measure, overlooked the fact, that there is such a thing in theoretical explanation as a false, as well as a real, simplicity. The simplicity of Brown's system, as has been previously shewn, was

attained by assuming phenomena to be analogous, or rather identical, and therefore referrible to one generic principle, which every one's common sense recognises to be widely different, and referrible to several distinct powers. His Excitability was not a power added to, but a power intended to comprehend and to supersede those previously recognised by physiologists as appertaining to the animal economy; a power in itself sufficient for the production of all the phenomena of that economy; not merely a power indispensably necessary to the other powers, which in various combinations are engaged in the performance of the several functions. It was in this respect, as has, indeed, been already remarked, that the Excitability of Brown differed so materially from the Cerebral Energy of Cullen. Whether correctly or incorrectly, Dr Cullen conceived that, in the animal economy, not only are the functions in which mind shares dependent on the nervous system, but that even those functions which are common to the animal with the vegetable economy, and on which the maintenance of the individual, and the continuance of the species depend, are, in the former, essentially dependent for their execution on an influence derived from that same system.

The only argument by which the Italian Brunonians seem to have attempted to establish the identical character of the various phenomena of the animal economy,—whether as manifested in the simple textures, such as muscular and nervous fibre, or in the more compound organs, such as the stomach, the liver, the kidneys, &c.,—is, that they are all produced by the operation of stimuli. But, on the same ground, it would be easy to show that the inflammation of gun-



powder by a spark, and the solution of salt by water, are merely varieties of *excitement* proceeding from the action of their reciprocal causes on the *excitability* of the substances in which the phenomena of combustion and solution are respectively manifested. For it is obvious that the signification attached by these writers to the term stimulus, is simply that of an agent capable of acting on a subject; as the signification which they attach to excitability is that of a subject capable of being acted on by an agent, or, in other words, susceptible of impression.

When the opposers of Brown, instead of denying simply the existence of such a generic property as excitability, argued that it was not one and indivisible, but different in different textures and organs, it is obvious that they multiplied the errors of the original system, and laid the foundation for the supposition, afterwards gone into by Blumenbach, Blane, and others, that each organ or class of organs is endowed with a peculiar vital power, in virtue of which it is adequate to the performance of its particular function or class of functions. However available, therefore, as an argument against Dr Brown's peculiar doctrine, the notion of a multiple excitability may be,—it is, in fact, as erroneous as that of an excitability single and indivisible.

In connection with the opinions entertained by some of the Italian physiologists respecting the property of excitability, two other speculations may be noticed; the first, the doctrine of Reproducibility, by which some of Brown's Italian supporters, and particularly Medici,\* endeavoured to account for the restoration of excitabi-

\* *Commentario intorno alla Vita.*

lity, which his theory represented as subjected to perpetual diminution ; and the second, the distinction which Tommasini attempted to establish, in his Lectures on Physiology, between the *act of life* and the *processes preparatory of life*.

It has been well shewn by Bergonzi, (Confronto, &c.) that what Medici, under the title of reproducibility, considered as a second vital property additional to excitability, and the laws of which he was at much pains to enunciate, can be regarded as nothing else than a general expression for a supposititious single cause of the phenomena well known to, and often described by, physiologists, under the name of Nutrition—those phenomena or functions by which, as Dr Cullen expressed it, “ seeing that the animal body from a small beginning grows to a considerable size ; and, at the same time, from the period of the birth, during the whole of after life, the body suffers, by various means, a daily and considerable waste,—the increase of bulk must be acquired, and the daily waste supplied.” It was the same phenomena which led Testa to recognise the possession, by what he calls the primary and secondary systems, or, in other words, the textures and organs, of Organic or Automatic powers, as distinguished from their Specific powers ; and it is to the same phenomena that Bufalini applies the term “ process of Organic Assimilation,” of which, he says, the purpose is to preserve the integrity of the organism.

With regard to Tommasini’s speculation respecting the distinction to be drawn between the act of life and the processes preparatory of life, it seems impossible to peruse his own account of his views, without arriv-

ing at the conclusion, that this distinction, if admitted, would exclude what, in the Galenical division of the functions of the animal economy, are termed the natural and the vital functions, from the *act of life*, and range them among the processes merely *preparatory of life*, leaving what are called the animal functions alone as the representatives of life, or, in the language of Brown, of excitement. It is unnecessary to point out how much confusion would arise from so arbitrary and artificial a distinction. Suffice it to remark, that the adoption of this distinction would render it necessary either to allow to vegetables the possession of the properties of irritability and sensibility, and such other powers as are concerned in the production of the so-called animal functions, or to exclude vegetables from the living kingdom of nature, as exercising processes preparatory of life, but not manifesting any tokens of the act of life itself. But the fact obviously is, that digestion, animalization, and nutrition, which Tommasini regards as merely processes preparatory of life, are the most decided manifestations of the existence of that state itself.

2. A question which has been very fully discussed by Italian pathologists, is, Whether the animal fluids, and more especially the Blood, can be considered capable of undergoing primary morbid alterations, in consequence of the direct agency of morbid powers upon them; or whether it is to the solids, and the properties with which the solids are endowed, that all the various morbid conditions of the economy are primarily referrible?

Of those Italian pathologists who, in adopting more

or fewer of the speculations of Brown, recognised, with Cullen, the extensive influence of the nervous system in the production of both the healthy and the morbid phenomena of the animal economy, some were disposed to regard the Humors as wholly incapable of undergoing *primary* morbid alterations; and, consequently, as incapable of constituting, by their vitiations, the proximate causes of diseases. Other Italian pathologists, again, were disposed to believe that the humors do occasionally, though much less frequently than used to be supposed, undergo changes not referrible to previously vitiated actions of the solids, and that these humoral alterations, when induced, give rise to more or less extensive disturbances of the economy.

Vacca,—who himself conceived not only that the humors do become vitiated in diseases, but that the greatest part of diseases depend on such vitiations, though acknowledging ignorance of the nature of these,—had, in the work already referred to, (p. 355 of this vol., note 1.) made Dr Cullen's opinions on the subject of the humoral pathology objects of his especial animadversion. The charges which he brings against these may be referred to two heads; 1st, Dr Cullen's having recognised the possibility of the occurrence of putrescency, or a tendency to putridity, in the animal fluids, and assigned excessive pathological importance to this state; and, 2d, his refusing a due pathological importance to other morbid conditions of the fluids. We shall enter the more readily on an examination of these charges at this place, from no occasion having previously occurred for particularly noticing the opinions which Dr Cullen entertained respecting the share which

the fluids have in the production of diseases,—a subject so curious in itself, and which, from the progress of animal chemistry, is now daily acquiring a higher degree of importance.

But before noticing these charges particularly, it may be remarked, generally, in regard to Vacca's criticisms upon Cullen, that they are mostly founded on the strange misconception noticed by Scuderi, (see p. 352 of this vol.) of regarding Cullen's theory of fever as his theory of all diseases. In enumerating the various celebrated physicians who have "flattered themselves with the notion of being able to establish, on their physical and anatomical knowledge, principles of doctrine from which a just and well devised plan of curing diseases might be deduced," Vacca concludes his list in the following terms, "And finally, Cullen, from spasm and atony of the nerves and vascular system, and from putridity of the circulating fluids;" and in several subsequent passages this author has employed language implying that Cullen recognised no other proximate cause of disease besides atony and spasm of the nerves and vascular system, and putridity of the humors (p. xvii. ; also p. 146).

That Cullen recognised the possibility of putrescency occurring in the living fluids, is undeniable ; and therefore, if Vacca's very able argument against the possibility of this occurrence, as stated in his work on *Putrid Diseases*,<sup>1</sup> first published in 1781, be admitted to be well-founded, he must be regarded as entitled to the superior commendation on this head. But it is a total misrepresentation of the fact to speak, as Vacca does,

<sup>1</sup> *Considerazioni intorno alle Malattie dette volgarmente Putride*, Lucca, 1781.

of Cullen's being prejudiced in favour of the doctrine of putridity (p. xxiv.), or to allege that it formed a prominent feature of his pathological system. Not only is no mention made by Dr Cullen of putridity of the fluids, in treating of any other form of disease besides fever, but he was at great pains to point out that the morbid phenomena which he was disposed, with previous physicians, to attribute to putrescency of the fluids, do not occur in all fevers, and consequently cannot be considered as primary elements of fever; and that in the cases in which these phenomena do occur, it is at such a period of the progress of the disease as to show that the putrescency must be regarded, not as the cause, but the effect of the febrile state, under the controlling influence of external powers. Vacca acknowledges, indeed (p. xxi.), that Cullen represented putridity, not as the cause, but as the effect of fevers; but instead of giving him the benefit of this declaration, he represents it as an *involuntary concession*, made under the necessity of avoiding contradiction with other favourite doctrines. A reference, however, to the several paragraphs of the First Lines, in which Dr Cullen discusses the causes, symptoms, and treatment of putrescency of the fluids, as a possible occurrence in fevers, will at once shew what Dr Cullen's views on this subject actually were, and what is the amount of Vacca's misrepresentations of them. In paragraph lviii., Dr Cullen declares that he by no means intends to deny that the cause of fever frequently operates upon the fluids, and particularly produces a putrescent state of them. "I acknowledge," says he, "that this is frequently the case; but, at

the same time, I maintain that such a change of the fluids is not commonly the cause of fever; that very often it is an effect only; and that there is no reason to believe the termination of the fever to depend upon the expulsion of the putrid matter." In paragraph lxxii., he mentions a putrescent state of the fluids as an effect of the circumstances which occasionally vary the appearance of typhus. "The ancients," he says, "and likewise the moderns, who are in general much disposed to follow the former, have distinguished fevers as putrid and non-putrid. But the notions of the ancients on this subject were not sufficiently correct to deserve much notice; and it is only of late that the matter has been more accurately observed, and better explained. From the dissolved state of the blood, as it presents itself when drawn out of the veins, or, as it appears, from the red blood being disposed to be effused and run off by various outlets, and from several other symptoms to be hereafter mentioned, I have no doubt, how much soever it has been disputed by some ingenious men, that a putrescency of the fluids, *to a certain degree*, does really take place in many cases of fever. This putrescency, however, often attends intermittent, as well as continued fevers; and, of the continued kind, both the Synochus and Typhus, and all of them in very different degrees; so that whatever attention it may deserve in practice, there is no fixing such limits to it as to admit of establishing a species under the title of putrid." In paragraph cv., Dr Cullen gives the promised enumeration of symptoms denoting the putrescent state of the fluids, as being, "1. With respect to the stomach, the loathing of animal food, nausea and vomiting, great thirst, and a desire of acids. 2. With respect to the fluids, the blood drawn out of the veins not coagulating as usual; hæmorrhage from different parts, without marks of increased impetus; effusions under the skin or cuticle, forming petechiæ, maculæ, or vibices; effusions of a yellow

serum under the skin. 3. With respect to the state of the excretions, fetid breath; frequent, loose, and fetid stools; high coloured turbid urine; fetid sweats; and the fetor and livid colour of blistered places; and, 4. 'The cadaverous smell of the whole body.'" Lastly, from paragraph ccxxii. to ccxxvi., Dr Cullen states the measures to be had recourse to with a view to obviate or to correct the tendency of the fluids to putrefaction.

Assuming the phenomena enumerated by Dr Cullen actually to occur, two distinct questions obviously arise with respect to them: 1st, Whether they are referrible to morbid conditions of the fluids, and particularly of the blood? and, 2d, If so, whether this morbid condition be actually of the nature of putrescency, or a tendency to putrid decomposition?

Many attempts have been made in recent times to ascertain what are the precise changes which the blood undergoes in fever, in its physical properties, and in its chemical constitution; as well as to determine at what period of the fever these changes occur, and consequently their relation, as antecedents or consequents, to the other morbid changes occurring in the economy. The observations of Clanny,<sup>1</sup> Burne,<sup>2</sup> Andral,<sup>3</sup> Stevens,<sup>4</sup> Lecanu,<sup>5</sup> Tweedie,<sup>6</sup> and others, leave little

<sup>1</sup> Clanny, Lecture on Typhus Fever.

<sup>2</sup> Burne on Typhus or Adynamic Fever. London, 1828.

<sup>3</sup> Andral, *Resumé de quelques recherches sur l'état du système sanguin dans la Fièvre Typhoïde*. Journ. Hebdom. vi. 334. 1830. See also *Clinique Médicale*, and *Essai d'Hématologie Pathol.* Paris, 1843, p. 61 seq.

<sup>4</sup> Stevens, *Observations on the Healthy and Diseased Properties of the Blood*. 1832.

<sup>5</sup> Lecanu, *Sur le Sang Humain*. 4to. Paris, 1837.

<sup>6</sup> Tweedie, Art. Fever. Cycl. of Pract. Medic. vol. ii.



room to doubt that the blood frequently, if not generally, exhibits, in cases of fever, both during life and after death, an extreme degree of fluidity. So far as it is possible to judge from the present state of the inquiry, there seems reason to believe that the blood may exist in an extremely fluid state, independently of any degree of putrefactive decomposition, in consequence of changes in the proportions of its ordinary constituents. But there seem some grounds, also—not as yet, it must be confessed, very extensive—for believing that the process of putrefactive decomposition may, in fever, commence in the blood while still circulating in the living body, as indicated by the development of ammonia.<sup>1</sup>

A strong presumption in favour of the occurrence of putrescency in some of the diseases of the animal economy, or of the dependence of these diseases upon putrescency, has been derived from the experiments of MM. Gaspard,<sup>2</sup> Leuret,<sup>3</sup> Trousseau and Dupuy,<sup>4</sup> Magendie,<sup>5</sup> Bouillaud,<sup>6</sup> Gendrin, and others,

<sup>1</sup> See Denis, *Essai sur l'Application de la Chimie à l'Etude Physiologique du Sang de l'Homme*, &c. 8vo. Paris, 1838. p. 201.

<sup>2</sup> *Sur les Maladies Purulentes et Putrides*. Magendie's *Journ. de Physiol.* ii. 1, and iv. 1.

*Sur l'Introduction de diverses Substances dans les Artères des Animaux Vivans*. Ibid. v. 319.

<sup>3</sup> *Sur l'Alteration du Sang*, Thèse, &c., v. *Arch. Gener.* xi. 98. (May 1826).

<sup>4</sup> *Sur les Alterations du Sang considérées comme Causes, ou comme Complications des Maladies Locales*. *Arch. Gener.* xi. 373. (July 1826).

<sup>5</sup> *Traité des Fievres*. *Dict. de Medec. et Chirurg. Prat.* art. Putridité.

<sup>6</sup> *Remarques, &c., avec quelques Expériences sur les effets des Substances en Putrefaction*. *Journ.* iii. 81.

which seem to have established, that, by injecting different putrid matters into the veins, a state of disease is excited in the system which resembles Typhoid fever, and which is accompanied with inflammation, sometimes of a gangrenous character, and with hæmorrhagic effusions in various internal organs of the body. On opening, after death, the bodies of animals experimented on in this manner, the blood, we are told, has uniformly been found in a liquid, uncoagulated state, and the solids have run rapidly into putrefaction. Professor Liebig considers it obvious, that, “when putrefying matter or pus placed upon a fresh wound occasion disease and death, these substances communicate their own state of putrefaction to the sound blood from which they were produced, exactly in the same manner as gluten in a state of decay, or putrefaction, causes a similar transformation in a solution of sugar.” (Chemistry of Agric. and Physiol., p. 370.)

As to the second charge, Vacca (p. xxii.) represents Cullen as affirming that the fluids are not subject to any other morbid conditions besides putrescency; and blames him greatly (p. 143) for supposing that all the phenomena of diseases, and particularly of fevers, can be explained without admitting vitiations of the humors, which he himself regarded as the most fertile source of diseases. (See also *Meditazioni*, p. 22.) But, to give colour to this allegation, he finds it necessary to bring several charges of inconsistency against Cullen, as having recognised, in particular cases, the occurrence of vitiations of the fluids, which in the general he denied. Thus, he

alleges that Cullen is inconsistent in denying, in his forty-eighth paragraph, the occurrence of morbid consistency or tenacity of the fluids, and then using such changes for the explanation of phenomena that present themselves in the progress of fevers. It is to be observed, however, that, in the paragraph referred to, Cullen was merely stating his reasons for believing that lentor, or viscosity, prevailing in the mass of blood, &c., is not *the cause of the cold stage* of fevers and its consequences ; which reasons he founds on there being no evidence of any such viscosity *previously* existing in the fluids. The admission of the possibility of these or other changes *supervening* in the fluids, in the course or progress of fever, is not, therefore, precluded by the line of argument which he there pursues.

But Vacca, particularly and triumphantly, insists on Cullen's having contradicted his own general doctrine, in ascribing scurvy to an altered state of the fluids different from putrescency. The fact is, however, that Cullen never made any such sweeping allegation respecting the non-occurrence of any other vitiations of the fluids besides putrescency, as that which Vacca imputes to him. All the length he went was to maintain, that the kinds of vitiations which pathologists, up to that period, had been in the habit of recognising, had been established on insufficient evidence ; that the state of medical and chemical science at the time did not admit of this department of pathology being then placed upon a proper footing ; and that the solids, more especially the nervous system, perform a much more important part in the production of diseases than

had been recognised by those whose attention had been exclusively directed to the vitiations of the fluids, and, apparently, a much more leading and primary part than these vitiations.

That these were actually his views will, I conceive, be rendered sufficiently evident by the following passages from his Lectures on Pathology, when treating of the morbid affections of the fluids as a part of the proximate causes of diseases ; and nothing inconsistent with the views there taken will, it is believed, be found in his practical writings. There was, no doubt, a difference in the comparative importance which Cullen and Vacca were disposed to assign to the influence of vitiations of the fluids in the production of diseases and the phenomena of diseases ; but no one who is acquainted with the recent progress of animal chemistry can suppose that Vacca understood any better than Cullen in what the vitiations of the fluids, to which he was disposed to attribute a larger share of pathogenical importance, actually consisted. If the commencement of knowledge is the getting rid of error, the observations which Dr Cullen made upon the particular changes that had been recognised by former pathologists, as capable of occurring in the living fluids, such as Spissitude, Tenuity, and various kinds of Acrimony, were well calculated, by the exposure they furnished of the insecure foundations on which this body of doctrine rested, to lead the minds of his hearers to investigate the subject with more rigorous accuracy, so far as the state of chemical science would at the time permit.

In proceeding, in his Lectures on the Institutions, to

consider the morbid affections of the fluids as a part of the proximate causes of diseases, Dr Cullen acknowledged that "we have often as little access to know the true state of the mass of blood, by our senses at least, as we have to know that of the nervous fluid." "Diseases," said he, "properly consist in the *actiones læsæ*, and, therefore, there may be a great many deviations in the state of the fluids from their most healthy state, without their producing diseases; it is only when the solids are at length affected, that they form diseases. I take notice of this just now, to shew that the pathologists have been very improperly busy in considering the affections of the fluids. That they have a share in the natural functions, and that their deviations have a share in diseases, is true, but they are the most inconsiderable part of them; and it is, indeed, the *actiones læsæ* that form the principal part of diseases." In speaking of the Baglivians, as having first insisted, "with much use and great propriety," upon the importance of the powers of the *fibra motrix*, Dr Cullen adds: "But if they maintained that the fluids were *always* very exactly adapted to the state of the solids, and were never to be considered as the cause of the disease, they pushed the matter to an excess. But, again, it is certain that the fluids are not so variable as has been imagined. The *vitia fluidorum* do not occur so frequently, and we are equally uncertain with regard to these deviations when they take place. I agree that this neglect of the fluids may be pushed to excess, but there is much justice in carrying this neglect to a great degree. This matter, however, is to be studied." "I am sorry that upon our present subject I am obliged to play the sceptic so much; but the present state of our science admits of no other conduct, and I think, therefore, that it is the safest." "I say that our fluid (the blood) is a mixt (compound), but chemistry has taught us nothing that is exact with regard to its ingredients, or their proportions; and every thing that has been

said with regard to this subject, is upon a very precarious footing." "I have just to say, what I would wish you to take along with you, that we know extremely little with regard to the mixture (composition) of animal fluids; and that whenever we apply our suppositions to the different ingredients, or their different proportions, it is purely hypothetical."

In taking a review of the morbid conditions of the fluids, recognised by pathologists under the names of Spissitude, Tenuity, and Acrimony, Dr Cullen stated the grounds of his belief, that the doctrines of the humoral pathology, then prevailing, were not available in medical science.

"I have," says he, in speaking of Spissitude, "endeavoured to shew that the actual occurrence of this state is uncertain, and that the means by which it can be produced are equally so. Therefore it will appear that the doctrine of Lantor, which rests upon that of spissitude, is upon the most uncertain foundation. When I first became acquainted with physic, I found that physicians reasoned boldly on this matter. They spoke of thickening or thinning the blood with as much confidence as a Scotch maid would speak of making porridge thicker or thinner. There is a possibility of the deviation of the blood towards spissitude, but it is both uncertain what causes can produce it, and to what degree it can be carried. Probably there are powers in the economy that serve to balance all the several causes of it, (such as the diminution of the secretions, and the inviscating of the more fluid parts,) which are means of preventing a morbid spissitude from occurring in the body." And in respect of the opposite condition of the blood, Tenuity, he remarked: "When we perceive increased secretions and hydropic effusions taking place, we must not refer them always to the tenuity of the blood, for in most instances they depend upon some change in the state of the secretory organs, or in the state

consistent theory with regard to it ; and you must, at your leisure, take the facts and make a cautious use of them.”<sup>1</sup>

Dr Cullen was accustomed to wind up his review of the doctrine of Acrimony of the humors, as expounded by Gaubius, in the following terms :—“ Gaubius has promised that he was to explain the different species of acrimony, but you see what it amounts to,—a very doubtful supposition with regard to an acid entering the mass of blood, and of a volatile alkali present there, &c. ; and it is evident that we know only of the presence of a neutral, and that we still know nothing of any other.”

Before quitting the subject of the Pathology of the Fluids, it may be remarked, that Dr Cullen fully understood that we are not at liberty to draw inferences respecting the changes which particular agents, introduced into the living economy, will induce on the circulating blood, from what we observe these agents to occasion when brought into contact with it, out of the

<sup>1</sup> The following passage from the same lectures, seems deserving of being quoted here, as recognising the inaccuracy of an opinion that was inadvertently admitted by Dr Cullen into the definition of Scurvy given in his Nosology, and which definition has, incautiously as I think, been referred to by a distinguished pathologist and practitioner, as shewing the uselessness of Nosology. “ There is no doubt, that if either the urine, or the perspiration by which the ammoniacal salts are thrown out of the body be diminished, a quantity of these salts will remain ; and we know, in the case of the Scurvy, that it is always assisted or increased by a suppression of perspiration. This has, indeed, been pushed to excess, by a late writer, who alleges that no scurvy is produced in the warm climates where the perspiration is kept up ; but there are facts to the contrary ; facts which prove that in the middle of the torrid zone, a saline putrescent animal diet will give the scurvy, though not so readily as in the colder climates, or in the same degree.”

body. The illustration which he gives of this fact, whatever may be thought of his mode of explaining it, is interesting and important in itself; and, so far as I am aware, original.

“With regard to the *Spirituosa*,” says he, “I have a curious observation to make. Alcohol applied to our blood does coagulate it; but this coagulation never takes place in the living body, because alcohol, when diluted to a certain degree, does not shew its powers in that way, and it cannot be introduced by the mouth into our fluids, without receiving such dilution. But we have instances of persons who take spirits in extraordinary quantity; and one would expect that among them we would find the blood coagulated; but on the contrary, their blood is of a remarkable fluidity. All our drunkards, male and female, particularly those who have dealt in brandy, have their blood more fluid than natural, and are liable to hæmorrhages; and I never knew a woman who was a drunkard, but she continued her menstruation longer than the ordinary period of life, even to 60 years of age; or, if it had gone away, it returned so late as that period and flowed again. I have not been extremely troubled in practice with cases of abundant menstruation; but towards the decline of life it is an accident that does happen, and it has occurred three times in such women as drank spirits, for once in those that did not, so that we are mistaken in our reasonings, in this respect; and this effect is not owing to the spirit acting upon the mass of blood, but to its acting upon the nervous system.”

3. The Brunonian canon, that the action of all the powers which operate upon the animal economy, whether with a noxious or with a salutary effect, is *stimulant*, was assailed in Italy upon various grounds. In the first place, it was maintained that there is a class of substances which exert upon this economy, *directly*, and *positively*, an action quite the reverse of stimulant,



whence they may be fitly designated as *contro-stimuli*. This, or an analogous term, it may be observed, seems to have been employed incidentally by John Hunter, in his Treatise on Inflammation, and by some other medical writers previously to the time of Rasori; but that ingenious physician,—the translator, it will be remembered, of Brown's Outlines in 1792,—appears, from his own statement, to have taught the doctrine of contro-stimulus, in the sense just specified, from the Chair of Pathology in Pavia, in 1796 or 1797; and to have been prevented from giving it more extensive publicity, up to the time of his account of the fever of Genoa in 1799-1800, only by the civil disturbances then going on in the north of Italy.

Considerable difference of opinion has been expressed by Italian pathologists, both as to the correctness and as to the originality of the belief, that there exists a class of substances capable of producing upon the economy effects the reverse of those of stimuli, in a direct manner,—that is, without previously producing an over degree of excitement, and thereby a state of what Brown termed *indirect* debility; and also without withdrawing, as in the cases of blood-letting, purging, low diet, &c., a portion of the stimulant agents habitually operating upon or within the system, and thereby inducing what Brown termed *direct* debility. So far as regards *noxious* contro-stimulant action, some traces of such a belief may be found in the opinions respecting poisons, and their antidotes, entertained by the ancients. “It is highly consolatory for the lover of truth,” says Emiliani (*Risposta*, p. 92), “to reflect that this opposite manner of action of certain substances to the operation of stimulus, was long ago known to the

ancients, of whom it certainly cannot be said that they were prejudiced in favour of these modern doctrines. They were acquainted with the contro-stimulant power of Aconite, inasmuch as they knew that it is a cold poison, and recommended generous wine as its antidote.\* In later times, Dr Cullen may be considered to have recognised the existence of morbid agents possessed of directly contro-

\* The author continues :—"Thus Macrobius writes (*Saturnalia* vii., cap. 6)—'Si quis aconitum nesciens hauserit, non nego haustu eum meri plurimi solere curari,' inasmuch as this wine, as he says, 'veneno frigido repugnat.' And Pliny, the Naturalist, taught to the same purport (*Lib. xxiii.*, cap. 23)—'Merum quidem remedio est contra cicutas, coriandrum, aconita, viscum, meconium . . . contraque omnia quæ refrigerando nocent ;' and, in the same sense, he adds in another place, that castor assists considerably in overcoming the poisonous nature of Aconite."

"The same opinion was entertained respecting Cicuta, as may be seen from some of the passages quoted. Of it, Androcides wrote to Alexander (see the 14th Book of the Plinian History, chap. 7—'Cicuta hominum venenum est, cicutæ vinum.' This is explained in the 25th Book, chap. 95, where speaking of the same plant, he says—'Remedio est, priusquam perveniat ad vitalia, vini natura excafactoria ;' as may also be read in the 5th Book of Dioscorides, chap. 11, and in the Symposian Questions of Plutarch, and elsewhere. More generally still, Celsus (*Lib. v.* cap. 27), using these remarkable words in respect of poisons—'Necessarium est exsorbere potionem meri vini cum pipere, vel quidlibet aliud quod calori movendo est. Nam maxima pars venenorum frigore interimit.' And this opinion must even have been generally received by the vulgar, since St Ambrose, in his book *De Utilitate Jejunii*, exclaims, 'Etiam major vis vini quam veneni.' So, without the knowledge of the ancients, the same vulgar, for a great length of time, administered to the poisoned Mithridate and Theriac, which remedies, as every one knows, derive the whole of their virtue from opium. These truths, sanctioned as they are by time, should be esteemed of much value in the practice of the art."

stimulant powers, in the operation which he attributed to the *specific* causes of Fever,—marsh miasm, and human contagion; for he conceived these to operate by directly diminishing the excitement of the nervous system, or by inducing, in greater or less degree, the state which he termed Collapse.

Of the *Therapeutical* agents, recognised in former times as possessing direct and positive powers of reducing morbidly increased action in the animal economy, the most familiar example is to be found in the so-called sedative operation of narcotics. Dr Chiaïis,\* indeed, in observing that the name of *Contro-stimulus* is given to any substance which acts in a manner opposite to a *Stimulus*, and lessens or destroys the effect of an exciting action, maintains that these powers ought not to be considered as identical with sedatives, seeing that stimuli may act as sedatives. Thus, he remarks, convulsions arising from *Asthenia* may be allayed by opium, a stimulant agent; whilst a *contro-stimulus* may increase, instead of allaying, the convulsive motions, by increasing the debility. But in these remarks, Chiaïis has probably not sufficiently attended to the distinction which, as has been already seen (vol. i., 419 to 426), Dr Cullen was so careful to draw between the sedative means that are efficacious in diminishing increased motion in the sanguiferous system, and those that remove increased motion affecting more entirely the nervous and muscular systems. If any substance can properly be said to act both as a stimulus and as a sedative, it must be because it exerts one

\* De nonnullis Contra-stimuli Theoriæ notionibus, &c. Nicæ, 1808.

of these modes of action upon one part of the economy, and the other mode of action upon another part, not because it exerts both modes of action upon the same part. Such, for example, is the case in respect of opium, which is stimulant to the sanguiferous system, but sedative to the nervous and muscular systems. Dr Cullen, indeed, seems to have thought, that, even on the sanguiferous system, the direct and positive operation of opium is sedative or contro-stimulant. For in speaking, in his *Materia Medica*, of the sedative influence of narcotics, and shewing that their effects, commonly and especially, first appear in the *animal* functions, he notices that, at the same time, the effects of narcotics appear also in the *vital* functions so far that the motions of these are weakened, and the frequency of their action rendered less; "and although," adds he, "this, from certain considerations, may not constantly appear, there are innumerable experiments which prove that a diminution in the action of the vital functions is frequently, and even commonly, the effect of narcotics." The admitted fact also of narcotics, instead of always proving sedative, or diminishing the action of the heart, often seeming to be powerfully stimulant with respect to this organ, and in their first operation often increasing the force and frequency of its action, Dr Cullen explains as an effect of *indirect* stimulation. Sacchi very properly recognised the distinction between the power of opium, in stimulating the action of the heart, and its soothing operation on the brain and nerves, and regarded this as a proof, that there may be in the same agent two principles of action, ope-

rating differently on parts dissimilar in their nature. (Lib. cit., p. 39.)

Any one who takes a general view of the multiplied reasonings and speculations of the Italian physicians on the action of Contro-stimulants, will have little difficulty in arriving at the conclusion, that the agents to which contro-stimulant powers are ascribed, correspond with those which other physicians, especially in this country, are in the habit of regarding as antiphlogistic agents. This, indeed, is denied by Spallanzani, who maintains that Contro-stimulant and antiphlogistic are not synonymous terms. This may be admitted ; but it is nevertheless certain that, in the view which the Italian practitioners have taken of Contro-stimuli, it is the agency of these substances in diminishing the action of the sanguiferous system that has chiefly, if not solely, engaged their attention ; and, accordingly, Mantovani points out, as the rule by which we are to judge of the contro-stimulant operation of medicinal powers, the analogy of their effects upon the economy to those produced by the detraction of blood. Thus understood, it must be acknowledged, that Dr Cullen's threefold enumeration of the sedative means to be employed for diminishing increased motion in the sanguiferous system, does not comprehend any directly contro-stimulant medicinal agents.

We shall have occasion to speak of this class of remedies more particularly when we come to consider the changes introduced in Italy into the Brunonian system of medical practice. Meanwhile, it may be noticed as a singular fact, that, in discussing the no-

velty of the doctrine of Contro-stimulus, no allusion seems ever to have been made to the account published by Dr Hamilton, of Lynn Regis, in 1795, of "A Successful Method of Treating Inflammatory Diseases by Mercury and Opium ;" or to the treatise published two years later by Dr Lind, in which he ascribed the beneficial operation of mercury in inflammatory diseases to its being possessed of directly antiphlogistic powers.

According to the supporters of the doctrine of Contro-stimulus, stimulant and contro-stimulant agents mutually counteract each other's operation on the economy, in virtue solely of their amount of stimulant or contro-stimulant energy. But to this general principle,—which has received from several Italian pathologists the title of the doctrine of Compensation,—it has been alleged, that some limitation is necessary, for two reasons. The first is, that certain contro-stimuli are more efficacious in counteracting certain stimuli, and certain stimuli in counteracting certain contro-stimuli, than other agents of the same classes respectively are ; whence it would seem that the operation of these agents does not depend simply and wholly on their degrees of stimulant or contro-stimulant power ; and, in the second place, the action of certain contro-stimuli is counteracted by the action of certain other agents which usually manifest analogous, that is, contro-stimulant powers. It was to the establishment of the latter of these points that the experiments of Sobrero, Stellati, and Bergonzi, were directed ; whilst opposite results, though these were necessarily of a negative character, were ob-

tained in the experiments performed under the direction of Tommasini.

A second ground, besides that of the existence of Contro-stimuli, on which the uniformly stimulant operation of all the powers which act on the animal economy has been opposed, is, that various therapeutical agents exert a specific, or rather, perhaps, an elective or eclectic action upon particular organs, as diuretics upon the kidney, emetics on the stomach, and purgatives on the intestinal canal.

Rasori, indeed, in a dissertation on Diuretics, read in the University of Pavia in the year 1797, and of which he gave a summary in one of the notes to his account of the Genoa Fever,\* had endeavoured to prove that, correctly speaking, there are no such therapeutical agents, that is to say, that there are not any remedies, the action of which being exercised particularly on the kidneys, increases the secretion of urine. In support of this view, he stated that those remedies which seem to operate as diuretics in one case of dropsy, and to cure it, no longer operate as such in another case, and instead of curing it, produce noxious effects. Besides, he argued, those very cases of dropsy which are cured with some one of the most reputed specific diuretics, as, for example, Foxglove, may be cured with equal security and facility by other remedies not reputed either diuretic or

\* Storia della Febbre Petecchiale di Genova Negli Anni 1799 et 1800. Di G. Rasori. P. 177 of Third Edition. This production is reprinted in the Raccolta di Opere Mediche Moderne Italiane, Tomo Nonno. Bologna, 1829. Pp. 257-365. The note referred to is given in pages 282 and 283.

specific, viz., all the purgatives and emetics, watery drinks, nitre, &c. ; in short, by all the contro-stimuli, and still more by the direct debilitants.

Though Rasori's argument was more particularly directed against the existence of remedies specifically diuretic, and meant to establish the proposition, that the substances which have been considered as such, all act either as stimulants, or as contro-stimulants, it was intended also to lead to the inference, that the same is true of all remedies which have been supposed to exert a specific action upon any of the particular organs of the body. The reasoning by which the argument is maintained, partakes of a certain amount of probability, and derives at least apparent support from the evidence of facts. In instances, for example, of what is called inflammatory dropsy, in dropsy after scarlet fever, in several of the acute forms of dropsy connected with granular and fatty degeneration of the kidney(*steatosis*), in certain instances of dropsy connected with *bronchitis*, *peripneumony*, and certain diseases of the heart, and even in some of those connected with the early stage of disease of the liver, while the medicines to which diuretic properties have been ascribed, exert no influence, those remedies which act in abating inflammation often seem to remove the dropsical swellings; and when the administration of diuretic remedies is followed by the disappearance of dropsical swellings, this result takes place, not so much in consequence of any peculiar action upon the kidneys as by their influence in abating vascular congestion and inflammation, one of the consequences of which then is, the



re-establishment of the action of the kidneys which had been suspended.

It may, on the other hand, be argued that there exists a number of substances which, when taken into the system in the healthy state, increase the amount of the urinary secretion, and which operate in the same manner in many of its morbid conditions. Whether these agents exert a specific or peculiar action on the circulation of the kidneys, is a question not of very easy determination. Among the saline substances it may be observed, that while the salts of soda and magnesia act mostly on the intestinal circulation, and those of ammonia mostly on the skin, it is a peculiar character of the salts of potash, that most of them evince a sort of preferential action on the kidneys, and this preferential or eclectic action of the potash salts is one of the strongest arguments in favour of the principle of a specific or peculiar action. A similar argument may be deduced from the action of certain essential oils and articles containing these oils, several of which, as oil of turpentine, oil of juniper, seem to excite the action of the renal vessels, and from the influence of diluted spirits and nitrous ether. Of all these substances it may be predicated, that they stimulate both generally and locally, and that, when given in such minute and graduated doses as to enter into the veins and be mingled with the blood, they exert their stimulant powers in exciting the renal vessels. But it may still become a question, whether the renal vessels are the only ones thus excited; for many of these agents act unquestionably upon the vessels of

other organs at the same time. It is thus that the specific diuretic power of substances designated Diuretic has come to be called in question by many intelligent physicians.

The uncertainty of the evidence of the action of diuretics had been remarked by Cullen ;\* and indeed it is a circumstance which cannot fail to attract the attention of practical physicians. It has hence resulted that there is among not a few a tendency to regard the agents called Diuretics as merely belonging to the class of general Excitants, and that the existence of a peculiar order of specific Diuretic agents is still in the state of a problematical question.

While these concessions must be made to the doctrine proposed by Rasori regarding the non-specific character of diuretic medicines, it must on the other hand be allowed that the apparent diuretic operation of several medicinal agents depends, as has been already said, very much on the peculiar state of the system in different forms of dropsical disease. It is not difficult to conceive that there may be circum-

\* Another example of the same kind that may be alleged, is with respect to medicines said to promote urine. That there are medicines of such a power, every body knows ; but, at the same time, every practitioner will allow that it is an effect which he often fails in producing, though employing the medicines recommended for that purpose by *Materia Medica* writers ; and it may be suspected, that, in very many of the instances in which they ascribe this virtue to medicines, they have proceeded upon a false experience, or perhaps upon none at all.

*A Treatise of the Materia Medica.* By William Cullen, M.D., &c. Vol. I. Edinburgh, 1789. 4to. Chapter Second, p. 147.

stances in which the operation of these substances is not sufficiently energetic to overcome counter-acting influences. Nor can it be any impeachment of their diuretic powers, that many cases of dropsy may be cured by evacuations different from diuresis. If, indeed, dropsy were always one and the same morbid condition,—a determinate amount of excitement induced by the operation of a certain amount of exciting or stimulant power on a certain amount of excitability,—it might reasonably be expected that it should require great uniformity of treatment ;—that the same remedies which remove it in one case should remove it in all ; and that there should be only one mode of operation by which remedies could prove serviceable. But the very different morbid conditions which give rise to that common result which is termed Dropsy, fully account for the variety observable in the action of the same remedies in different cases, and for the variety of remedies that may prove serviceable in the same case.

A doctrine in such direct opposition, as that proposed by Rasori, not merely to the experience of medical practitioners, but to the most familiar observation of mankind, was not likely to find much favour, with however much subtlety it might be urged, or however formidable an array of difficulties in the established opinions relative to the specific power of particular substances to act upon particular organs, might be adduced in its support. It seems scarcely proper, therefore, to single out any one individual as having more particularly resisted the doctrine of the non-specific operation of medicinal agents, and as having maintained the

possession, by different classes of substances, of powers that cannot properly be referred to the common property of stimulation, and the possession, by the different organs of the economy, of susceptibilities not referrible to the common property of excitability. Vacca and Strambio both pointed out the existence of such specific therapeutical agents as a fatal objection to the Brunonian doctrine of a single and indivisible excitability susceptible of only one mode of impression. But it may be remarked that Bondioli, in the Inaugural Lecture which he delivered on being called to the chair of Materia Medica at Pavia, in the year 1803, insisted very forcibly on the great diversity of morbid states to which the economy is liable, and of noxious powers by which these are produced; and thence inferred that there must be a corresponding diversity in the agents by which these morbid states admit of being removed.

4. The term Diathesis, which, in the progress of the discussion of Brunonianism, rose into so much favour among the Italians, is one to which, in the progress of medical science, very various meanings have been attached. In particular, it has been used by some to denote a disposition or constitution of the economy, congenital or acquired, often latent and imperceptible, imparting to it a tendency to take on some particular form of disease. When these tendencies or predispositions of the economy are examined, they are found to have reference to certain *families* of diseases, such as Fevers, Inflammations, Hæmorrhages, &c.; so that, used in this sense, there

are probably as many *Diatheses* as there are natural families of diseases. In a similar sense the term Diathesis has been employed both by English and French physicians to denote not only the disposition to particular diseases and forms of disease, as the Catarrhal Diathesis, the Rheumatic Diathesis, the Gouty Diathesis, but the tendency to certain definite forms of new deposition and subsequent disintegration, affecting the whole system or certain parts of the system. Thus the Strumous and the Tubercular Diatheses are familiarly spoken of, and occasionally medical writers mention the Cancerous Diathesis. None of these, therefore, can be the signification in which the term Diathesis is employed, when it is alleged that all diseases are referrible to two diatheses ; or when it is, at most, disputed whether there may not be a third diathesis.

It has already been shewn that the dual diathetical division of diseases adopted by Brown, was merely a new mode of expressing a fact which had long been recognised by pathologists, viz., that in some diseases there is evidence of increased, and, in others, of diminished, force of action in the economy. The practical question to be determined was, whether the evidence of this increased or diminished force in the economy is to be found in the exercise of all its functions, or in that of some of them only ; and if the latter, in which of them it is to be looked for ? Now, the consideration which determines, in the eye of the practical physician, whether a disease is one of increased or of diminished force, or of the sthenic or asthenic diathesis, seems to be chiefly, as has, indeed, already been

pointed out, (see p. 261 of this vol.), the state of the circulation, and the influence exerted over it by particular means of treatment. Excessive vigour of circulation, restored to a natural state by antiphlogistic measures, determines a disease to be of the sthenic Diathesis, or a disease of vigour. Languid circulation, restored to a natural state by tonic and stimulant remedies, determines that a disease is of the asthenic diathesis, or a disease of debility.

In conformity with the representations here made, Rolando remarks that these two dynamic states of the human body, viz. augmentation of force or increase of vigour, and debility and languor, have been recently designated under the names of hypersthenic and hyposthenic Diatheses, principally in circumstances in which there is observed an increase of the cardiaco-vascular excitement.

The diatheses of Sthenia and Asthenia, considered as two opposite states of a single and identical property on which all the phenomena of the economy depend, or rather, perhaps, as two opposite results of the identical operation of external agents on such a property, share in all the objections, physiological, pathological, and therapeutical, to which such a doctrine of the economy is exposed. But if considered as significative of opposite states of certain functions of the economy, capable either of being induced suddenly by the operation of morbid agents, or of arising in the progress of diseases, and materially influencing the character which these diseases exhibit, and the mode of treatment which they require, the sthenic and asthenic diatheses are matters of fact highly deserving

both of theoretical investigation and of practical attention.

According to Brown, the production both of the sthenic and of the asthenic diatheses is attributable to the action of stimuli, in excess or in defect. But Rasori,—in following out the doctrine of the existence of two opposite classes of powers or agents, capable of operating upon the economy, the one class acting as stimuli, and the other as contro-stimuli—was led to think that the two Brunonian diatheses may respectively proceed in a direct and positive manner from the operation or agency of appropriate powers, the sthenic diathesis from the operation of stimuli, and the asthenic diathesis from the operation of contro-stimuli; and that they ought, consequently, to be respectively designated as the diatheses of Stimulus and Contro-stimulus.

The full consideration of this subject, however, would require more space and time than it is possible here to bestow upon it; and, after all, it may become a question whether it is truly worth the labour.

5. It seems obvious that a division of diseases, of which the vigour of the circulation is assumed as the basis, must necessarily be threefold; 1st, Those in which the circulation is vigorous in excess; 2d, Those in which it is defective in vigour; and, 3d, Those, if such diseases exist, in which its vigour sustains little or no departure from its natural or ordinary amount. If the state of the system common to the diseases of each of the two first of these classes respectively, be represented by the name of Diathesis,

under the titles of *Sthenia* and *Asthenia*, Stimulus and Contro-stimulus, &c., then the state of the system which occurs in the third class of diseases, and which is obviously marked by negative qualities as regards the circulation, may be represented either as *Adiathetical*, *i. e.*, destitute of particular diathesis ; or, if we can detect any common character in the diseases that fall to be comprehended under it, that common character may, perhaps, be assumed as the foundation of a third Diathesis. The common character of this third Diathesis, however, cannot have reference to the vigour of the circulation ; and it seems very doubtful whether we are likely to discover any positive character in which the diseases attended by neither increase nor diminution of the vigour of the circulation, will all correspond, or by which they can be distinctly characterized.

Guani imagined that he had detected the groundwork of a third diathesis, in what he designates *Irritation*, as distinguished from Increased or Diminished Excitement.\*

In diseases of *Sthenia* and *Asthenia*, according to this pathologist, the restoration to health is to be accomplished in the one case by diminishing, and in the other by increasing, the amount of excitement. But there is a class of diseases, he alleges, in which recovery does not take place till, by the efforts of nature, assisted more or less by the physician, some noxious matter is expelled from the system. Such is the case

\* *Riflessioni sull' Epidemia della Liguria, Ossia Saggio di una Nuova Teoria sulle Malattie Epidemiche e Contagiose. Genoa. Anno X, (1801.)*



in smallpox, for example ; and such is the case in all diseases depending on the presence of a contagion or miasm. The necessity for this expulsion arises from the impossibility of these substances, whether generated within the body, or introduced from without, being assimilated with, or incorporated into, the living textures. The action of these substances is neither stimulating nor contro-stimulating. It may be called Irritant ; the substances themselves Irritant Powers, and the diseases which arise out of them, Irritative Diseases, or diseases of the Irritative diathesis. But a disease which is irritative may, at the same time, be sthenic, or it may be asthenic, as in the case of smallpox already referred to. Hence the irritative may coexist with either the sthenic or the asthenic diathesis. Further, it is to be remarked that there are therapeutical agents, as well as morbid powers, which operate as irritants ; not a few medicinal substances, as well as morbid causes, seeming to belong to the class of Heterogeneous and Irritant stimuli.

It cannot fail to be remarked that the character which Guani has assigned to his irritative diseases, and which was suggested to him by the fever of Liguria, corresponds very closely with the view which Dr Cullen had taken of the final cause of fever in general, in pronouncing it to be "an effort of nature to remove something that is noxious, and which by its continuance would disturb the system, or destroy it." Previously to the appearance of Guani's essay, Vacca had alleged that *chronic* diseases, in general, do not consist in a degree of excitability (excitement) greater or less than what is healthful, but, for the most part,

in a derangement or disorder, of an unknown nature, of the vital organs, produced by hurtful matters introduced into the organism ; and if these be not expelled, or corrected and neutralized, he says, the diseases never terminate favourably. Hence, he further inferred, the general plan of cure proposed by Brown with so much confidence and boasting, not only cannot be secure and advantageous, but must be always uncertain, and not unfrequently pernicious.

Admitting that there are certain diseases in which the characters embodied by Guani under the title of Irritation, display themselves, and others in which they do not, then this evidently might furnish the basis of another dual division of diseases into the Irritative and Non-irritative. But unless it could be shewn that the Irritative comprehend just those diseases which are not included under the diathesis of *sthenia* and *asthenia*, it plainly cannot serve as an element of a threefold division of diseases into the *Sthenic*, *Asthenic*, and *Irritative*, such as he sought to establish ; and, that this is not the case, is manifest from Guani's own statement, that the Irritative Diathesis may coexist with either of the other two.

In the progress of the discussions which arose in Italy on the subject of the Irritative Diathesis, the term Irritation came to be regarded as the expression of a third mode of morbid exercise of the functions, besides increase or diminution of vigour, that, namely, of irregularity or perversion. Accordingly, we find Guani, in his *Essay on the Irritative Pathological Condition*, quoting, with satisfaction, an expression

of Galen, to which Brera had referred,\* as involving the idea of a third pathological condition,—noticing that the Hippocratic school had previously shadowed out the triple division of morbid states,—and referring to Gaubius, as recognising an alienated vital force, as well as one in excess and one in defect. We have already had occasion in this volume (p. 285) to notice Dr Cullen's threefold division of the diseases of the Moving Fibres into those in which contractility and motion are diminished; those in which they are too strong, or too much increased; and those in which there is irregularity of motions; and his corresponding division of the remedies of these diseases into Stimulants, Sedatives, and Antispasmodics; by which last term, he declared, he meant such medicines as allay, or remove, irregular motions in our system. It is obvious, indeed, that the deranged performance of a function, whether simple or compound, must necessarily fall under one or other of these three general heads, namely, increase, diminution, and perversion or irritation; the last being terms sufficiently extensive to comprehend all alterations different from increase or diminution.

\* “*Si vires supersunt, si desunt, si quidam mali effectus interveniunt.*” *Della Doctrina Medica Del Controstimolo. In Raccolta di Opere Mediche Moderne Italiane. Tomo v. Bologna, 1827. Pp. 95–98.*

The passage here quoted by Guani as occurring in Galen, is found in Celsus, lib. iii., cap. 5. Correctly, it is “*Seu supersunt vires, seu desunt, seu quidam alii effectus interveniunt.*” It appears to be Guani who has altered the passage, and who has committed the mistake regarding its exact source.

It deserves also to be remarked, that it is in explaining the treatment of Fevers that Celsus thus expresses himself.

I cannot take leave of this part of the subject without remarking, that the diseases, which Guani ascribed to the existence and effects of Irritation, correspond to that extensive tribe of disorders which physicians are in the habit of ascribing to the presence and action of a morbid poison, whether received into the system from without, or generated within the economy. Thus the diseases which result from the action of Miasma or Malaria, as Ague, Remittent Fever, Yellow Fever, Dysentery, Cholera; those which are ascribed to the action of Smallpox, Scarlet Fever, Measles, the Syphilitic Virus, and probably several of the Chronic Cutaneous disorders; and those which are believed to be the result of the impure air of jails, ships, and crowded habitations, may all come under the head of Irritative Diseases, such as they are described by Guani.

In the progressive advancement, however, of pathological observation, in the revolutions of medical theory, and in the course of practical experience, it has become common, especially among the physicians of France and England,—and some of the divisions of Germany,—to employ the terms IRRITATION, IRRITABILITY, IRRITABLE, and IRRITATIVE, in a sense something different from that which was attached to these terms by Guani and his Italian followers. To this subject, which has acquired considerable importance among modern physicians, we may probably have occasion subsequently to direct the attention of our readers.

## 6. The Brunonian division of diseases into the

Sthenic and Asthenic, was represented as inaccurate on another ground, viz., that, in the progress of a disease, the Sthenic Diathesis may be replaced by the Asthenic, or *vice versa*.

Professor Ambri, of Parma, seems to have been the first person who expressly undertook to shew, that the Diathesis, which prevails in any particular disease, is not permanent, but is liable to change in the progress of the disease, or, as he terms it, to undergo Transmutation ; and, consequently, that diseases cannot be distinguished from one another by their belonging exclusively to one or other of the Brunonian Diatheses ; nor can be treated without a reference to those exchanges or transmutations of the Diatheses, or of the predominating states of vigour and debility, which frequently take place in their course, and render an alteration in the mode of management proper and necessary.

The conversion of one disease into another, or, perhaps, more properly speaking, the disappearance of one morbid affection on the appearance of another,—as in the cases of Rheumatism, Gout, and Erysipelas,—had, indeed, been treated of by various authors, as Rodericus a Castro, Gianella, Hoffmann, Lorry, and Ferriar ; but the question of Diatheses, as understood by Brown and his followers, did not, of course, fall under the scope of their consideration, so that the terms Transmutation of Disease, and Transmutation of Diathesis, are by no means to be considered as synonymous.

That the Diathesis of a disease may change in its progress, is a principle which had been fully recog-

nised by Dr Cullen, in the establishment of one form of fever,—under the title of Synochus,—in which the symptoms, at the commencement, manifest an inflammatory or sthenic, and, in the course of the disease, a nervous or asthenic character. In fact, his arrangement of fevers fully shews that the Inflammatory and the Nervous, or Typhous states, constituted, in his estimation, so far, at least, as the PYREXIÆ are concerned, those opposite conditions of the economy by which the treatment pursued must mainly be regulated.

7. It was objected, likewise, to the Brunonian view of diseases as referrible to opposite Diatheses, that diseases cannot be regarded as, at any given period, simply sthenic, or simply asthenic ; for that one part of the economy may be in a state of *Sthenia*, while another is in a state of *Asthenia*. Thus, in the course of fevers, and of some other diseases, the nervous and sanguineous systems may be in opposite conditions of vital action.

Sacchi had denied (p. 79) the correctness of Brown's assertion, borrowed, as he says, from Themison, that *Sthenia* and *Asthenia* cannot co-exist in the same economy ; and that, consequently, roborants and debilitants cannot be required in the treatment of the same patient, or that there cannot be contra-indications ; and Strambio had affirmed, that we often see combined, in the same subject, those diseases which Brown regarded as of opposite natures. But Giannini may, perhaps, be considered as the person who, in Italy, had the merit of establish-

ing most fully the principle, that, notwithstanding the connection and reciprocal dependence of the several parts of the economy on one another, they may, and actually do, under certain circumstances, and particularly in fever, exist in very different conditions of vigour of action.

According to Giannini, the systems of the living machine, practically considered, may be reduced to three—viz., the Nervous, the Arterial, and the Muscular. In these three systems, and in their several dependencies, there may occur a loss of balance, by one or other of them acting with excessive, while the others act with deficient force, so as to produce in the economy the simultaneous existence of states of over and under excitement, or of vigour and debility. This complication of the opposite Diatheses depends, Giannini conceives, on the nervous system experiencing a degree of debility in which the other systems do not participate, and is named by him *Neurosthenia*. He farther states, that, whilst the nervous system is always affected in *Neurosthenia*, it is not necessary that both the arterial and the muscular systems should be implicated; for the one or the other of these systems separately, or even a part only of one of them, may be affected.

These views of Giannini must be considered as having been, in a great measure, anticipated in the account given by Dr Cullen, of “the circumstances which express a combination of irritation and debility, particularly as the irritation occurs in the brain, and gives a suspicion of those topical affections which, after death, we find to have truly hap-

pened, though symptoms of the inflammatory state had not happened to appear before.”\*

8. On the question of the grounds of distinction between Universal and Local Diseases, some observations have been already made in a previous part of this volume; (pp. 64–70). Little doubt can be entertained that the question is one of considerable difficulty; for when we examine the list of diseases denominated *Local*, with the most cursory view, it will appear that scarcely one of them is justly entitled to the character of being Local, in the correct sense of the term. Previous to the time at which Cullen published his Nosology, Sauvages gave a class designated VITIA, including many diseases then generally supposed to be Local. The VITIA of Linnæus includes a multifarious collection;—Abscesses and their effects; various diseases of the bones, of the skin, of the bloodvessels; Displacements, Deformities, and Spots of various kinds. Vogel’s class of VITIA includes those which are visible on the surface of the person, and is not less multifarious and comprehensive than that of Linnæus; but Deformities constitute a class by themselves. Sagar, again, presented, in the VITIA, seven orders, embracing MACULÆ, EFFLORESCENTIÆ, PHYMATATA, EXCRESCENCES, CYSTS, DISPLACEMENTS, and DEFORMITIES.

When Cullen defined Local Diseases to be those affecting part, not the whole, of the human body, he

\* See Works, vol. i., p. 589; and Life, vol. ii, p. 262–264.



stated simply a fact which may be admitted as correct. But though the disease manifests itself as an affection of one part only, it does not follow, that this manifestation may not be dependent upon the condition of the system at large, or that of some one of the General Tissues, the Vascular or Nervous System, or of the Blood itself. Cullen at the same time remarked, that it is often doubtful whether certain diseases ought to be referred to the class of Universal or Local ; and he argued, that, constituted as this class is, many diseases may be more easily arranged than in the systems of other nosologists. He allowed, also, that this class might comprehend the VITIA of other nosologists ; but though the VITIA correspond with many of the local diseases, he takes care to say, that the VITIA are not to be regarded as synonymous with the LOCALES.

These Local Diseases include ;—I. Disorders of Sensation, DYSÆSTHESIÆ ; II. Derangements in Natural Appetites, DYSOREXIÆ ; III. Disorders in certain organs of Movement, DYSKINESIÆ ; IV. Excessive Evacuations, APOKENOSES ; V. Retentions, EPISCHESES ; VI. Tumours ; VII. Displacements, EKTOPIÆ ; VIII. Breaches in Continuity, DIALYSES. It would not be difficult to shew that scarcely one of the diseases arranged under these respective eight orders is justly entitled to the character of local, even as defined by the author. Many of them are either effects and symptoms, or parts, of general disorders ; some form diseases of the organs of the senses,—the eye, the ear, the nose, as in the case of Cataract, Amaurosis, Staphyloma ; the several

forms of defective vision, *Dysopia*, and *Pseudoblepsis* ; various forms of Deafness, and loss of smell and taste. *Bulimia*, *Polydipsia*, and *Pica*, almost always are symptoms ; and Nostalgia was, even in the time of Dr Cullen, known to be a symptom of a peculiar form of mental disorder ; for Sagar, in his system, published at Vienna in 1776, not only places it among the MOROSITATES or Depraved Appetites, and in the class VESANIÆ, but, in his definition of it, informs the reader that he himself, when in Croatia, laboured under the disorder, with aversion to food, constipation, and general dropsy ; yet, from this condition, he recovered without any medical treatment or medicine, by merely returning to his native soil. Cullen himself allows that *Nostalgia* can by no means be regarded as a Local Disorder.\* It is manifest, indeed, to any one who reads the Notes on the different genera of this class, that Cullen was aware of the difficulty of this part of his task, and the defects of the arrangement proposed ; for he allows that *Anorexia* and *Adipsia* are in all instances symptomatic ; and, from the great number of instances in which other *Genera* of the Local Class are specified as Symptomatic, it is manifest that he felt the difficulties rather to increase than to diminish. *Aphonia* is very generally a symptom ; and even of the three species enumerated, *A. gutturalis*, *A. trachealis*, *A. atonica*, the two former are manifestly symptoms and effects of other dis-

\* *Nostalgia sola, si quidem reverâ morbus sit, minime pro locali haberi potest. Nosologia Method., Cl. iv., Ord. ii., p. 318.*

eases. The same may be said of *Paraphonia*, of *Dysphagia*, and of *Contractura*.

Among the EPISCHESES, or Suppressions, Constipation is either a symptom of Indigestion, or scarcely a disease. Ischuria is a complaint so various in origin, that, though often dependent on mechanical impediments in the urinary passages, yet in the case of the Renal form, it is manifestly a mere symptom of disease of the Kidney, or disease of the Brain. Amenorrhœa is now always referred to disease of the uterus, or disease of the system at large. Among the TUMOURS, Aneurism and Varix belong to the diseases of the Arteries and Veins ; and, in the instance of the former, it is well known that the tumour derives its origin from a general morbid condition which affects the whole arterial system.

In the eighth or last order of the LOCALES of Cullen, namely, the DIALYSES, are placed three diseases, *Herpes*, *Psora*, and *Tinea*, two of which are diseases of the skin ; while the third, *Tinea*, is known to consist in a peculiar morbid state of the roots and bulbs of the hair. Of the two Cutaneous Disorders, *Herpes*, as now understood, is not a local affection, but is the external indication upon the skin of a morbid state of the system at large, often associated with gastro-enteric irritation. *Psora*, again, or *Scabies*, is probably to be viewed as a malady affecting the skin, in consequence of the presence of certain minute parasitical animals burrowing in the membrane,—often connected with a disordered state of the system at large ; and in one instance, the *Scabies cachectica*, certainly connected with impaired general health,

either as an effect or as a concurrent and simultaneous condition.

It may here be remarked, that the most serviceable mode of considering the diseases referred to the Class LOCALES, is by referring them to the several organic textures in which they are seated ; because, while in this method nothing is said whether any given disease is Local or not, the attention of the student is directed to the main fact of the anatomical and physiological character of the disorder.

These admissions I think it quite indispensable to make, for two reasons. The first is, that though they shew the defect of the Cullenian arrangement and distinctions, they also prove that Cullen only failed where no other person could succeed. The services which Cullen rendered to Nosology are sufficiently great, to claim the acknowledgment and homage of the profession, supposing the whole of the fourth class to be entirely given up. It was not in the nature of things that he could succeed ; for the attempt was made without perfect and full knowledge of the morbid forms which the nosologist thus attempted to place in methodical order. Pathological investigation has shewn that many of these Diseases called Local, including several Chirurgical Diseases, are intimately connected with some general state of the system, and are to be regarded as mere external expressions and indications of that general state ; such, for instance, are Aneurism, Skirrhus, and Cancer, several of the varieties of Ulcer, and Caries, when connected, as often it is, with the Strumous Diathesis.

The second reason for admitting the imperfection

of the Fourth Class of Cullen's Nosological System is, that it may be seriously doubted whether any disease, though apparently local, is really unconnected with some state of the general system. All the parts of the body, however small, and however remote, are connected by means of the vascular system and the blood, by the most intimate and direct ties; and they are connected by a relation not less intimate, though perhaps less direct, by means of the nervous system. The vascular system and its contents are connected with the alimentary organs by means of veins and lymphatics; and it cannot be unreasonable to infer, that the contents of the alimentary canal acting upon the state of the blood and the nervous system, must exert an influence upon the most remote and the most minute parts of the economy. If this influence exists, and is operative in the state of health, it must be not less so in the state of disease; and we, therefore, must admit, that it is a legitimate deduction to infer, that it is difficult to conceive any disease which is altogether local, and which is not connected with the system at large. Now, it must be allowed, that Cullen shews that he quite understood this mutual and intimate connection of all the parts of the animal machine;\* and if, in the establishment of the class of Local Diseases, he seems to have departed from it, it was probably only because the connection seemed as to these disorders to be less conspicuous, and more readily to escape from superficial observation. Since

\* See the Works of William Cullen, M.D., &c., edited by John Thomson, M.D., &c., volume i., p. 436. Edinburgh, 1827.

his time, however, it is impossible to deny, that, upon this subject, views more comprehensive, and more accordant with what seems to be fact, have been taken by medical observers.

John Abernethy, following in the footsteps of John Hunter, shewed that not a few diseases, usually regarded as Local, owe their origin and continuance to some morbid state of the general system ; and any one who is either familiar with the patients of a large hospital, or reads what has been written during the last forty years by surgical writers, will have little difficulty in arriving at the conclusion, that, as pathological knowledge has considerably diminished the number of diseases called Local, and long considered and treated as Local, so it may diminish them in a degree still greater, until it may become a question whether there be such a thing as a disease strictly Local.

It has been shewn by Sir Benjamin Brodie, that diseases of the joints, though appearing as local affections, are connected with a morbid state of the system at large. Every practical surgeon knows how difficult, how impossible it may be said to be, to treat ulcers by mere local management. Various tumours of the Mamma, various affections of the Uterus, cannot be successfully treated merely as local disorders. Not only is Skirrhous now known to be dependent upon a state of the general system, but Encephaloma or Soft Cancer, Melanosis or Black Degeneration, and Colloid Growth, there is strong reason to believe, are in the same predicament ; and this list, further inquiry, it is not improbable, may enlarge.

These observations bring me to the answer of the question stated under this head; and this is, that if there are proper grounds for the distinction between Universal and Local diseases, neither observation nor experience have succeeded in discovering these grounds. One disease, a few diseases may be less general than others; but none can be said to be strictly local and unconnected with the system at large.

As to the other division of the question, whether the circumstance of being attended or unattended by Diathesis, constitutes this distinction, when Diathesis is interpreted as used by Brown, the distinction vanishes into nothing, and becomes altogether unserviceable. On one aspect of this subject, namely, the necessity of a sufficient and good medical education, combining the full study of medicine with that of surgery, in order to make an accomplished and serviceable medical practitioner, several judicious observations were made by Dr Thomson so long ago as in 1813.\* Those, however, who wish to understand well the nature of the alleged distinction between Universal and Local diseases, will do well to study with attention the writings of the late Mr Paynter Vincent,† those of Sir Benjamin Brodie,‡

\* Lectures on Inflammation. Edinburgh, 1813. Introduction.

† Observations on Some of the Parts of Surgical Practice. By John Paynter Vincent, late senior surgeon to Saint Bartholomew's Hospital. London, 1847. 8vo, pp. 364.

‡ Lectures illustrative of Various Subjects in Pathology and Surgery. By Sir Benjamin C. Brodie, Bart., F.R.S., Serjeant Surgeon to the Queen, &c. London, 1846. 8vo, pp. 411.

Pathological and Surgical Observations on the Diseases of

those of Mr Macilwain,\* and the Lectures on Surgical Pathology by Mr Paget.†

9. Dr Brown's idea, that diseases differ from one another only in Diathesis and degrees of Diathesis, was combated by those who insisted on the importance of the *Forms of Diseases*, and of the *Pathological Conditions*, as objects of scientific inquiry, as well as by those who maintained, with Bufalini, the doctrine of *Particularism*.

Bondioli undertook to shew, that it is necessary for the physician to pay attention, not merely to the *Diathesis* prevailing in any universal disease, but also to the particular *form* which it exhibits. By this term, he seems to imply, that the practitioner must attend not only to the constitutional symptoms, as marking more or less vigour of the general economy, and, in particular, of the vascular system, the Joints. By Sir Benjamin C. Brodie, Bart., V.P.R.S., &c. Fifth Edition, with Alterations and Additions. London, 1850. 8vo, pp. 399.

\* *Medicine and Surgery One Inductive Science*; being an attempt to improve its study and practice on a plan in closer alliance with inductive philosophy, &c. By George Macilwain, Fellow of the R. M.-Chir. Society, &c. London, 1838. 8vo, pp. 551.

*Remarks on the Unity of the Body, with a view of Improving the Constitutional Treatment of Local Diseases.* By George Macilwain. London, 1836.

† *Lectures on Surgical Pathology*, delivered at the Royal College of Surgeons of England. By James Paget, F.R.S., lately Professor of Anatomy and Surgery to the College; Assistant Surgeon and Lecturer on Physiology at St Bartholomew's Hospital. Volume I. Pp. 499. Volume II. London, 1853. 8vo, pp. 637.



but also to the more local symptoms arising immediately from the organ principally affected. "What Professor Bondioli calls morbid forms," says Tommasini, "(connected with particular influences of different agents on certain parts of the body, and with certain modes of affection that result from thence), and which he very properly distinguishes from diathesis, are, if I mistake not, to be accounted *nosological*\* affections."

Bondioli conceived the system of Brown to err fundamentally in directing attention wholly to the diathesis prevailing in a particular disease, and withdrawing it from the *form* which the disease assumes. In this remark, Bondioli seems to allude to Brown's declaration, that it is enough to guide medical practice to know whether the disease is sthenic or asthenic, and in what degree one or other of these states or diatheses prevails; whereas the practitioner ought, in Bondioli's opinion, to form a more precise judgment as to the seat and intimate nature of the disease, since two sthenic or two asthenic diseases, equal in amount of diathesis, may require different modes of treatment, according to diversities in these other respects.

Bondioli justly observes, that physicians had, from the earliest periods of medicine, made these forms the particular objects of their attention; but that the physicians of our days had introduced the doctrines of Diatheses, and had substituted these in

\* "I give the name of Nosological," says the same author, "to those symptoms only which indicate what is the part affected, and the extent of the disease."

place of the proper consideration of the peculiar forms of disease.

Fanzago concurred with Bondioli in thinking that diathesis is not the sole object of consideration in diseases ; and in recognising the importance of attention to their *forms*, which term, he remarks, is not to be limited to the nosological character, but must include the whole history of the disease ; so that the attaining a knowledge of the form involves the necessity of both descriptive and historical analysis. But the principal object of Fanzago's Treatise on the Essential Differences of Universal Diseases, was to shew that, independently of the *diathesis* of Brown, Rasori, &c., and of the *morbid form* of Bondioli, the medical practitioner must take into consideration the *pathological condition*, or that particular temporary morbid process, which, in universal diseases, is commonly formed either in some organ, or in some system of the animal economy, but which is to be distinguished from permanent organic alteration, or serious disorganizing lesion of any part. It is, he holds, on the diathesis and pathological condition, or local affection, conjointly, that the form, or nosological character, of universal diseases must depend, and on the latter even more than on the former.

It seems obvious that Fanzago understood the term Pathological Condition as equivalent to the Proximate Cause of Cullen.

10. Another question that has excited a good deal of discussion among Italian pathologists, relates to

the grounds of distinction between Dynamic or Functional and Organic diseases.

The fundamental principle of the system of Pathology which Bufalini has so zealously laboured to substitute for the Brunonian, seems to consist chiefly in this, that it is the organization, and not any abstract property that is the seat of those phenomena in which health and disease consist ; that life results from the action of the appropriate stimuli upon the organization ; and that it is doubtful whether the organic mixture requires the addition of any particular matter to complete or perfect the organization.\*

Hence, in the Essay on Life which he published in 1813, and in which he commenced his attacks upon the dynamical doctrines of Health and Disease, he denied altogether the existence of dynamical or functional diseases, maintaining that all diseases are, in their first origin, organic ;—some, indeed, in which the morbid alteration of the organized structure is occult or invisible, and others in which it is evident to our senses ; but that there is no dynamical or functional derangement which does not depend upon a change in the *organic mixture* ; and that, consequently, there is no absolute or essential difference between Functional and Organic diseases, both depending upon change in the *organic mixture*.

These opinions Bufalini restated and illustrated in the first edition of his Analytical Pathology, published in 1819 ; in his Modena Prize Essay (1823) ; in his work entitled *Ciculate intorno alla Medicina*

\* Raccolta Di Opere Mediche Moderne Italiane, 1 Tomo. Bologna, 1827.

*Analitica* ; or Familiar Discourses on Analytical Medicine\* (1825); and in the 3d edition of his Pathology, printed at Pisa in 1828.

Bufalini contends (Patol. Analit., new edit., p. xxvii.) that the pathologists who may be called Functional, all admit material but imperceptible changes in the organization. The Functional derangements, therefore, are those in which the organic changes are imperceptible and temporary.

In opposition to Brown's views, Bufalini held it to be unphilosophical to consider a property as the subject of augmentation or diminution, without reference to the state of the substance of which it is the property ; and that in Pathology, consequently, it is a mistake to regard as disease mere changes in property apart from changes in structure. Regarding the body as an organic compound, the character of which is to be the seat of Life, and looking upon the essence of disease as belonging not only to the materiality, as he terms it, of the organic compound, he defines Disease to be " a special change of the material condition of the living body, induced either by unsuitable substances existing within the body, or by an alteration in the order and composition of the organic Compound ; or, in other terms, a change in the material condition arising from the effect of chemico-organic, or mechanico-organic action."†

\* *Cicalate* means literally pratings, babblings, gossipings. But as this conveys in English a meaning by which Bufalini would feel himself not honoured, the term is rendered as above.

† *Fondamenti Di Patologia Analitica di Maurizio Bufalini, Cesenate, Edizione Terzia. Pesaro, 1827. Tomo i., pp. 94, 95.*

The following information was obtained from the records of the [redacted] Department of the Army, [redacted] Office, [redacted] City, [redacted] State, [redacted] Country, [redacted] Date.

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\* *Remarks on a generalised discussion of Anæsthesia into  
"Anæsthesia and Sympathy." By William Macleod, M.D. Sur-  
geon to the Queen. Edinburgh Medical and Surgi-  
cal Journal, vol. lxxv., October 1851.*

1. *Amorpha canescens* (Mill.) B.S.P. *Amorpha* *canescens* (Mill.) B.S.P.  
 2. *Amorpha canescens* (Mill.) B.S.P. *Amorpha* *canescens* (Mill.) B.S.P.

stimuli ; but the abuse of these exciting agents, or their employment too long continued, gives rise to a state of debility. Indirect debility is nothing else, says Weikard, than an effect of exhausted excitability. The PYREXIÆ may proceed to the state of indirect debility, either in consequence of the violence of the Diathesis, or in consequence of the administration of stimulating remedies and by inactivity in the use of refrigerant and debilitating remedies.

When the definition of Indirect Debility is attentively examined, it results that Indirect Debility is in no sense or manner different from Direct Debility. The debility which follows the long-continued and the excessive use of what are called stimulants,—that is, distilled spirits, wine, stimulating articles of food,—is, in all instances, the effect of more or less disease. These articles, namely, spirits, wine, and similar substances, have at least two actions. The first is a direct action upon the mucous membrane, the muscular tissue, the blood-vessels and nerves of the stomach and intestinal canal, by which they produce an unnatural and hurtful irritation, which, often repeated and long-continued, injures the natural properties and probably alters the structure of these tissues. In the second place, they are absorbed by the veins into the blood, and being mixed with it, not only alter the properties of that fluid, but irritate all the textures through which the blood is circulated. In this course they seriously disorder the function of respiration. The deleterious effects of the introduction of spirituous or alcoholic stimulants into the system in impeding the function of respiration have been

demonstrated experimentally by Dr Prout. This physician ascertained by many experiments, that when wine, malt liquors, and similar articles, are taken into the stomach, for a considerable time after they have been introduced, indeed until by the action of the organs they are eliminated, less carbonic acid is discharged by the lungs than in the normal state.\* The inference resulting from this fact is, either that the blood when circulating through the ramifications of the pulmonary capillaries has lost some of its power of absorbing oxygen from the inspired air, and exhaling carbon, or that the pulmonary vessels are less capable than in the normal state of effecting the requisite aeration of the blood, and getting rid of the carbonaceous matter brought by the veins from the different parts of the animal body.

Then, they derange other organs and functions. Whence are formed disease of the inner coat of the blood-vessels in all the organs, diseases of the liver, diseases of the kidneys, of the pancreas, the spleen, and any vascular organ well supplied with blood. The liver nevertheless suffers first and most considerably in this irritation, in consequence of the direct connection of its vascular system with the veins of the alimentary canal. The debility therefore thus induced is the direct effect of morbid irritations.

\* Observations on the Quantity of Carbonic Acid Gas emitted from the Lungs during Respiration at different times, and under different circumstances. By William Prout, M.D., &c. *Annals of Philosophy*, Volume Second, p. 328. November 1813. And *Edinburgh Medical and Surgical Journal*, Volume Seventy-sixth, page 131. July 1851.

Nor is it different with regard to PYREXIÆ or Febrile diseases. Whether these diseases be considered as the effects of the operation of Poisons upon the animal economy, or simply as conditions of deranged and perverted action in the vascular system, it must be admitted as a legitimate inference, that their tendency is to induce irritation throughout the whole extent of the vascular system, and consequently in all the organs supplied by the vascular system. If we suppose, what seems most probable, that most of these diseases depend upon material subtle poisons, floating in, and conveyed by the atmosphere, whether Miasma, Malaria, or other principles, as those of Typhus Fever, Smallpox, or Measles, it is manifest, that they are in the first instance absorbed by the lungs and mingled with the blood of these organs; and thence circulated to all parts of the body. This will irritate not only the lungs and impede their function in depurating the blood, but the brain and spinal chord, by transmitting to these parts a poisoned and morbid blood. As, however, this at every respiration is increasing, unless means are adopted to counteract the effect thus resulting, a two-fold lesion is produced. The lungs are no longer able to depurate the blood received by them, and, consequently, blood imperfectly respired is sent to all the organs, thus poisoning them; and when these organs are thus deprived of their supply of properly respired blood, they cease to perform their functions in the complete and normal manner of health. The brain, spinal chord, and nerves, are loaded with dark-coloured, unrespired blood, and



hence nervous energy is impaired and gradually extinguished ; hence the great debility. The stomach, liver, spleen, and alimentary canal, receiving the same sort of blood, are not only disabled from performing their proper functions, thus constituting a most important cause of debility, but their capillary vessels having been irritated by poisoned blood in the early stage of the disease, become enfeebled and often give way as the lesion advances. In what sense debility thus induced can be called indirect, it is not easy to discover. It is the direct effect of a great and often irrecoverable injury done to the capillary vessels.

With regard to the states of debility ensuing at the close of Smallpox, Measles, Scarlet-fever,—these, morbid anatomy shews, are most commonly the effect of vascular congestion or inflammation in the tracheo-bronchial membrane and lungs, or in the kidneys and other internal organs.

According to the testimony of John Brown himself, the idea of Indirect Debility was suggested to his mind by reflecting on the state of the System in Gout.\* Now, though it cannot be denied, that, notwithstanding the subsequent research and inquiry of

\* “ Notwithstanding disease first, and afterwards death, are produced not by the privation, but by the abundance of those things by which life is sustained, he perceived that Debility was the cause of his disorder (Gout), and that the remedy was to be sought not in debilitating but strengthening measures. To this sort of Debility he thought proper to give the name of *Indirect*.”—Preface to *Original Work in Elements of John Brown, M.D.* By Thomas Beddoes, M.D. London, 1795. Vol. i., p. xviii.

more than sixty years, it is by no means clearly ascertained what is the exact nature of the deviation from health, when the system is under the influence of Gout; yet this much is known, that there is a relative excess of nitrogenous principles, compared with the demands of the system; that urea, uric or lithic acid, and probably other nitrogenous principles, are not excreted as in the state of health; that they are accumulated in the blood; that this accumulation depends upon larger amounts of nitrogenous articles being received into the system than are necessary, and diminished power in the secreting and excreting organs in effecting their expulsion and elimination; and that not so much this accumulation of nitrogenous materials, as the cause or causes of this accumulation, is the principal cause of Gout. That this accumulation of nitrogenous principles, though present, is not the cause of Gout, appears from this fact, that though these principles are neutralized in various modes, principally by the use of alkaline agents, yet the disease is not thereby prevented from appearing. It is only, in short, by either reducing the nitrogenous elements of food to the natural necessities of the system, or by facilitating and promoting elimination of those products which are in excess, that the disease can be moderated or prevented from appearing in its characteristic forms.

The condition now described may be called *Debility* in one sense, in so far as the action of the secreting and excreting organs is impaired and enfeebled. But whether it is more entitled to be called

*Indirect* than any other species of Debility, is a point which may be safely left to the judgment of the reader. As to the method of curing this debility, by whatever name it is, to be designated, by means of nutritious food, that is, highly nitrogenous food, the use of alcoholic and vinous liquors, and similar stimulants, opium and similar narcotics, it is exactly one of those pleasant, but pernicious delusions, one of those insidious courses, by which persons of a certain description have in all ages lured the credulous and unreflecting part of their fellow creatures to their own destruction.

Upon the whole, it may be justly concluded, that Indirect Debility, as taught by Dr Brown, is just such a creation of the fancy as might be expected to be made by a person entirely unacquainted with practice, imperfectly informed on the phenomena of disease in the living body, and quite ignorant of the destructive effects of disease in the dead subject.

Notwithstanding the objections now stated, several of the Italian physicians were disposed to believe in the existence of this sort of debility. Tommasini, in particular, so late as 1816–1817, admits that he does not absolutely deny the possibility of this morbid condition; but he thinks that it ought rather to be denominated *Debility relatively* direct or absolute, reducing it, in point of fact, to a diminution of excitement from insufficiency of the ordinary stimuli to maintain it.\* In illustration, he refers to the well-

\* Della Nuova Dottrina Medica Italiana. Prolusione alle Lezioni di Clinica Medica nella Pontificia Università di Bo-

known instance of a drinker so habituated to the strong stimuli of wine or spirituous liquors, that he becomes unsteady in his movements, and unfit for his ordinary duties, by giving up a little the use of these potations, and does not recover the vigour of his members until he drinks again of these liquors. "I recal here," he adds, "the intelligent remark of Gaubius, 'Hesternam crapulam nova pocula solvunt.' But I equally think that this condition, when an inflammation is not engendered, is reduced to an effect of habit and a need of strong stimuli, from the fibre not being in a condition to feel those which are moderate, and that in these cases the excitement may be sustained at the degree of health by applying stimuli adapted to the necessity, so that the vital conditions may be restored to their former state by diminishing, by very slow degrees, the daily use of the stimuli, and substituting in their place alimentary articles suitable for repairing in the fibre that which was lost. I am likewise of opinion that a distinction ought to be made between this state and the *ebriety*, which is also an effect of an excessive stimulus, and is curable by means of cold, with the use of contro-stimulant remedies. I am of opinion that the abuse of stimuli is very rarely followed by the indicated condition of *habitual necessity of stimuli*; it being too easy and too common, that, by the abuse now mentioned, there is created some acute or slow inflammatory process, under the force of which the laws

logna Per l'Anno Scolastico 1816-1817. Del Prof. Giacomo Tommasini. Bologna, 1817. 8vo. Nota 14, p. 77, republished in Raccolta di Opere Mediche Moderne Italiane. Tomo VIII. Bologna, 1829. P. 62.

of habit are speedily broken, and the fibre no longer endures either strong or moderate stimuli. The Diathesis, in short, is one of stimulus, and is not curable except by means of contro-stimulant remedies. This is, indeed, observed in drinkers, in whom, upon the development of the so-called *gastritis* or *hepatitis Potatorum* (the gastric symptoms of *Delirium Tremens*), wine becomes unendurable; the disease consists in a slow phlogistic Diathesis, and there is no mode of curing it unless in the use of remedies fitted to overcome the chronic inflammation.\* Above all, it must be remarked, that, in the greatest number of maladies, in which the disciples of Brown raise the cry of *indirect Debility*; there exist chronic inflammatory disorders already commenced, and which are to be cured only by the contro-stimulant method. This we have already had the means of demonstrating by facts in many patients admitted into this Clinical Establishment, in whom were united the causes and the characters of the Indirect Debility of Brown, and who, notwithstanding this combination, were happily cured by means of contro-stimulant remedies."

Though in some respects this is not very clear, chiefly from the writer speaking in abstract general terms, yet it is sufficiently so to shew that, upon the whole, Tommasini corroborates the correctness of the remarks already made upon Indirect Debility. It is here admitted, in express terms, that Indirect Debility is that state of feebleness so often observed in habitual drinkers, and which, if not directly dependent upon disease of the stomach, the liver, and

\* See Febbre Gialla Americana, Nota 30 & 35.

the brain, and the habitual impregnation of the blood with a poisonous agent, is, at least, in the greatest number of instances, associated with these morbid and unnatural states. It seems, after this, quite unnecessary to say more upon this imaginary form of Debility.\*

12. Though the question whether inflammation be ever of an Asthenic character, may seem to imply at first sight something frivolous and absurd, as if any disease were not an Asthenic process ; yet it is requisite to make a few observations, which may tend to elucidate, if not to solve, the question.

It will facilitate much the examination of this question, if it be stated in the following terms. Does Inflammation arise in an enfeebled state of the system, or in a state of increased strength ? Does it take place in circumstances which denote strength or debility, whether in the system at large, or in particular parts and organs ? Are there forms and varieties of the inflammatory process, which, when established, require to be treated, not by antiphlogistic, depleting means, which tend still further to reduce and diminish the power of the system, but by the use of tonics and similar agents, which are believed to have the effect of increasing the strength and energy, either of the system at large, or of particular parts.

\* It is a melancholy and humiliating fact in the history of this piece of Pathological Hypothesis, that the author of this fanciful sort of Debility was a very frequent subject of it in his own person.

First, then, all inflammations of every kind, and wherever seated, are of an Asthenic character. All inflammations take place under circumstances of impaired vigour; all inflammations impair and tend to impair strength; and none can with any correctness of language be said to give strength, or to increase strength; or, all inflammations are proofs of impaired strength, and take place only when the strength is impaired. But it was soon observed by physicians and surgeons, that different inflammations were accompanied with different degrees of loss of strength. Some inflammatory diseases, it is observed, are attended with little loss of strength; others are attended with considerable loss of strength; and certain forms of inflammatory disorder are remarkable for suddenly and quickly reducing the patient to a degree of feebleness threatening the speedy extinction of life.

It has been further observed, that in certain inflammatory attacks and diseases, the action, the power, or the energy of the vascular system, seems to be vigorous, and to indicate that the heart and arteries contract with strength. In others, on the other hand, the beats of the arteries are small, feeble, sometimes irregular and intermittent, and the energy of the animal and natural functions is greatly impaired. In instances of the former class, the inflammation has been said to be attended with symptoms of strong Reaction. In instances of the latter, the disorder is said to be attended with symptoms of weak and feeble Reaction. The latter, also, have been by various observers designated as Malignant

Inflammations (*Phlegmasiæ Mali Moris*); and some have gone so far as to call the former Benign Inflammations. It is manifest that no inflammatory disease can, without the greatest impropriety of language, be characterized as Benignant; and all that can be understood from this mode of speaking is, that one form of Inflammation evinces less malignancy than another.

It seems to be principally in reference to the difference now stated, that Brown and his Italian followers spoke of Sthenic and Asthenic Inflammation. Had they proposed to distinguish the different forms of inflammation according to the degrees of the Asthenic state with which they were associated, and according to the nature of the different organic textures in which these inflammations were seated, they would have made a nearer approach to nature and truth, and would have spoken in terms more intelligible of the subject.

The inflammatory diseases remarkable for impairing quickly and considerably the strength of the animal body generally, and the energy of its animal and natural functions, are the following :

1. Erysipelas, or St Antony's Fire ; that is,
  - a. Erysipelas of the Skin.
  - b. Erysipelas of the Mucous Membranes.
  - c. Erysipelas of the Serous Membranes.
2. Diffuse Inflammation of the Filamentous Tissue.
3. Inflammation of the Adipose Tissue ; a. Subcutaneous and Intermuscular ; b. Periangeal ; c. Perinephral and Omental ; d. Intraosteal or Medullary.



4. The disease which produces Acute Nekrosis, and which is often the same as the last mentioned.
5. Inflammation of the Inner Coat of the Veins ; (*Phlebitis Diffusa*).
6. Acute, Spreading, or Traumatic Gangrene.
7. Senile Gangrene, especially when connected with Disease or Inflammation of the Arteries.
8. *Enteritis* and *Peritonitis*.
9. *Enteritis* and *Peritonitis* in Strangulated Hernia.
10. Several of the forms of Puerperal Fever.
11. Gangrene of the Lungs, whether original or ensuing upon Pneumonia, or taking place in the course of Typhus Fever.
12. The poison of Glanders and Farcy, when it affects the Human Subject ; to which may be added the Black Blister or Malignant Pustule, and certain intense forms of Oriental Plague.

In all these forms of Inflammation, the strength and power of the muscular system are greatly impaired ; often delirium or typhomania is present ; the beats of the arterial system are feeble, small, and creeping ; and very often life is speedily extinguished.

These forms of inflammation were known to Bromfield and John Hunter. But the fullest accounts of them have been given by Mr James in his work on Inflammation ;\* Dr Duncan in his Essay on Diffuse

\* Observations on some of the General Principles, and on

Inflammation of the Cellular Tissue ;\* Dr Butter in his work on the Plymouth Dockyard Disease, or Irritative Fever ;† Mr Travers in his Treatise on Constitutional Irritation ;‡ Mr Nunneley in his work on Erysipelas.§ So recently, even, as 1852, short essays on this form of inflammatory disease have been published by two writers, one of whom styles it Unhealthy Inflammation, the other, Low Inflammation.|| But though these essays are proofs of the fact, that medical practitioners are quite aware that inflammation may arise under, and be attended with, symptoms of great debility ; they cannot be said to

the Particular Nature and Treatment of the Different Species of Inflammation, &c. By J. H. James, Surgeon to the Devon and Exeter Hospital, &c. London, 1821. 8vo.

\* Cases and Observations of Diffuse Inflammation of the Cellular Tissue. By Andrew Duncan junior, M.D., &c., Edinburgh Medico-Chirurgical Transactions, Volume First, p. 470.

† Remarks on Irritative Fever, commonly called Plymouth Dockyard Disease, with Mr Dryden's Detailed Account of the Fatal Cases. By John Butter, M.D., F.R.S., F.L.S., &c. Devonport and London, 1825. 8vo, pp. 302.

‡ An Inquiry concerning that Disturbed State of the Vital Functions, usually denominated Constitutional Irritation. By Benjamin Travers, F.R.S., Senior Surgeon to St Thomas's Hospital, &c. London, 1826. 8vo, pp. 556.

§ A Treatise on the Nature, Causes, and Treatment of Erysipelas. By Thomas Nunneley, &c. Leeds and London, 1841. 8vo, pp. 307.

|| An Essay on Unhealthy Inflammations. By M. Broke Galwey, Surgeon R.R. of Artillery. London, 1852 (Dec. 1851). 8vo, pp. 85.

A Practical Sketch of Low Inflammations. By S. F. Statham, Assistant Surgeon University College Hospital. London, 1852 (Sept. 25). 8vo, pp. 31.

present views by any means new to the majority of well-informed English physicians and surgeons.

It is generally understood that one leading mark of these forms of inflammation is, that they show a great tendency to spread or diffuse their action, and in this diffusive course they are attended with the effusion of serous fluid, not very coagulable ; in short, containing little or no coagulable matter. By this character they are supposed to be distinguished from ordinary inflammation, when it shows no tendency to spread, and when it is accompanied with the effusion of fluid containing so much coagulable matter, that it speedily undergoes coagulation, and then presents the product called Coagulable Lymph, Organizable Lymph, and Plastic Exudation. The former sort of inflammations, in short, are Disjunctive ; the latter are Adjunctive, Uniting, or Agglutinating.

There is no doubt that all these forms of inflammation take place in weakened states of the system, and must be regarded as proofs of the weakened state of the system, excepting only those instances in which they are manifestly the effects of the application of poisons to a wounded part, as in the instance of the bite of serpents, wounds received in dissecting dead bodies, and similar conditions ; and even in the instance of wounds received in dissecting dead bodies, the effects vary much, according to the state of health and strength of the individual. In the energetic and temperate, there is great resistance to the peculiar effects of the poison ; but in those of feeble and impaired health, or broken constitution, the accident is followed by most destructive effects, so that

even here the inflammatory action may be regarded as indications of a weakened state of the animal and vital powers.

The practical application of the distinction now made becomes highly important, in reference to the mode in which these forms of inflammation are to be treated. In ordinary circumstances, inflammatory diseases are allowed to be most successfully treated by the prompt and judicious employment of the antiphlogistic regimen, or by what the Italian physicians have denominated the Contro-stimulant method. In the treatment, however, of the Inflammations supposed to be Asthenic, and taking place in circumstances of weakness, and what various physiologists have called imperfect action of the Vascular System, this method requires to be modified in a very considerable degree. Blood-letting, though it has been commended by some physicians and surgeons, is pronounced by others to be a most deleterious practice. All agree in allowing the judicious but guarded uses of laxatives, aperients, and purgatives, not the drastic, but only to produce the ecoprotic effect. In former times, many physicians were partial to the use of Peruvian bark, and, recently, Quinine is believed to be not only useful, but indispensable. Local incisions and punctures are beneficial in the early stage of the disease, in order to stop inflammation and promote adhesion, and, in the latter, in order to evacuate matter. But it is of great moment to employ general and graduated tight bandaging. In the throat and mouth, the great remedy is a strong solution of nitrate of silver.

There are still other instances in which inflammation takes place in weakened states of the system or some of its parts, and in which the antiphlogistic treatment becomes either inadmissible, or requires to be much modified.

The inflammation which succeeds in parts which have been exposed to extreme cold, whether in the moderate form of Chilblain (*Pernio*), or the more intense one of Frost-bite, cannot be treated by antiphlogistic means, at least by those of a depletory character. Cold applied gradually, then friction, and the use of remedies which gently excite the circulation, as oil of turpentine, camphorated spirits, and tincture of opium, are the means which experience has found least hurtful and most efficacious.

There is still a most extensive tribe of Inflammatory, or rather Phlegmasioid disorders, which take place only in systems and in parts in which the indications of debility are evident. These are the affections called Strumous and Scrofulous. Scrofulous inflammations of the eye and eyelids ; of the skin and mucous membranes ; of the filamentous tissue ; of the glands ; of the joints ; of the bones, especially the cancellated bones ; of the brain and its membranes ; of the lungs ; of the peritoneum ; all take place in persons in whom there are other indications of debility, especially in the capillary vessels. It has been also alleged, that the constitution of the blood is altered in these diseases, and that this is the main cause of these inflammatory affections ; and various analyses of the blood of persons suffering under different strumous diseases have

been adduced in proof of the correctness of these views. It seems natural that the blood should be altered in constitution, and the nature of its constituent parts, in order that scrofulous disease should take place. But it may be further said, that this alteration depends upon, and proceeds from, a previous imperfection or disorder in the chylo-poietic or alimentary functions, because it is not easy to imagine the constitution of the blood to be much altered, except through the medium of the food, the manner in which it is digested, and the different states of the chylo-poietic organs. It is also to be observed that all the chemical analyses of strumous blood show no great difference from the constitution of blood reputed healthy; and, notwithstanding these analyses, little aid is afforded in conducting the treatment of this tribe of disorders. The principal conclusion to which the experience both of former and of recent observers has led, is, that these forms of phlegmasioid disease take place in weakened states of the system, and that their treatment requires the cautious and judicious employment of tonics, with proper regulation of the digestive functions.

13. In opposition to the opinion professed by Brown, that, in the Phlegmasiæ, the local inflammation ought not to be regarded as the cause, but merely as a part, or symptom of the general disease, Tommasini was led to the conclusion, that, in the fevers usually termed Idiopathic, as well as in those recognised as Symptomatic, local inflammation is the

primary disease. This important conclusion he expounded at considerable length in his *Treatise on Yellow Fever*, published in 1805.\*

In that treatise he expressed his conviction that pathologists and physicians had manifested, in general, too much deference for the word *Fever*, and had been too much in the habit of considering the state of fever as the primary disease, and of regarding those alterations which are the true cause, in the light of mere complications or effects of *Fever*. For his own part, he had always been inclined to think, that not only those fevers which have been preceded by inflammation of some part of the body, but those also which develop themselves at the same time as a local inflammation, are referrible to it as their cause ; and he had been disposed even to suspect, that, in the fevers in which we see inflammation of some part speedily succeeding to, or associating itself with, the febrile movement, this inflammation might have been kindled by the influence of the morbid causes, previously to its manifesting itself, and might be considered as the cause, rather than as the effect, of the general pyrexia. In short, he was disposed to regard inflammation as a fruitful source of serious hidden violent morbid affections, such as others had believed to have their origin in fever. The practice of the art had tended to confirm him in these suspicions. A fever which continues, without any apparent cause, for several days, which is accompanied with obscure and painful sensations in some internal cavity, or even with an uneasiness marking nothing precise,

\* See Appendix, Note P.

but which may cover the most serious derangements, led him readily to suspect the existence of an internal inflammation as maintaining this fever. Those obscure sensations, those pains or shootings, which are usually regarded as symptoms of fever, may, on the contrary, he thought, be immediately connected with a dull or latent inflammation, which is the cause of the fever ; and, in fact, says he, if such a fever long resists the aid of art, we see either pulmonary phthisis, or chronic enteritis, or peritonitis, &c., develop itself ; and if the patient dies of it, his body presents the traces and results of the inflammation which had not been suspected ; and that even at several points which had not during life exhibited any mark of disturbance. These disorganizations, of whatever kind they may be, which had not been foreseen, and which, in general, are considered as the consequences of a long and obstinate fever, and as secondary diseases, Tommasini regarded as the effect of the inflammation, the primary disease, which, far from being the effect of the general disturbance, has, on the contrary, given birth to it and maintained it. To these views, Tommasini mentions that he had been led by the observations of John Hunter, Frank, and Sasse, respecting inflammation of the bloodvessels.

Entertaining these opinions, Tommasini could not assent to Brown's dogma, that the only instances in which inflammation is the cause and not the consequence of a general affection, are those in which the inflammation is produced by a local and mechanical cause. He adduced various examples of inflammatory diseases, not produced by mechanical causes, in which the pyrexial affection seems to be secondary ;



and maintained that there is no distinction in the character of the inflammation by whatever class of causes it may have been produced,—being in all cases an alteration of a diffusible nature, or one which may extend itself to the whole organization.

The account which Dr Cullen gave of the Phlogistic Diathesis, evidently shows that he regarded it as a general affection of the whole system, giving a tendency to local inflammation. “Such a state of the system seems often,” he says, “to arise, and subsist for some time, without the apparent inflammation of any particular part.” “It does, however, appear, also, that the general diathesis frequently arises from inflammation begun in a particular part.” “In every considerable inflammation, though arising in one part only, an affection is communicated to the whole system, in consequence of which an inflammation is readily produced in other parts besides that first affected. This general affection is well known among physicians under the name of the Diathesis Phlogistica.”

The opinion that many universal diseases, and especially fevers, depend upon inflammation of some particular organ, was adopted, and still farther extended in its application, by Testa, in his Treatise on Organic Actions and Reactions, in which he maintained that all diseases are local in their origin; arguing (chap. ii., § 9), that if all the actions and reactions of the economy were altered similarly, and in the same proportion, this would not constitute a state of disease, but a different idiosyncrasy, or a different human economy.

“Every disease,” says he, “is, in its first commencement, nothing but a mere local affection, more

or less extended, in one of the primitive or of the secondary systems, or in more than one of the one or other." (2d Corollary, p. 141). "Fever, as regards its principle and its nature, is nothing but a symptom of parts primarily affected." (Cap. xi. § 2).

Opinions, in a great measure coinciding with those of Tommasini and Testa, were subsequently adopted and extended by M. Broussais in France, apparently without his being aware of this anticipation, and constitute, as is well known, one of the leading features or articles, as will presently be more particularly noticed, of what he has termed the Physiological doctrine of Medicine.\*

14. Though not immediately connected with the speculations of Brown, it is impossible to omit, from a view of Italian medicine as it existed at the commencement of the nineteenth century, the doctrine of Tolerance or Endurance of Medicinal Agents, advanced by Rasori, which holds so important a place in the therapeutical system of some practical physicians.

Rasori, besides satisfying himself that we possess, in contro-stimulant substances, such as Aconite, Fox-glove, and Tartar Emetic,—as well as in evacuant remedies, such as bloodletting and purgatives,—the means of curing any diseased state which proceeds from excess of stimulus, just as we possess in stimulants the remedies of the diseases which arise from contro-stimulus, came to believe that the economy is capable of bearing greater doses of contro-stimulant or of stimulant substances, in proportion as the

\* Appendix, Note Q.

stimulant or contro-stimulant diathesis, against which they are brought to bear, is more intense. He was even led to believe that this capability of the economy for bearing stimulant or contro-stimulant remedies, affords a criterion of the existence, and a measure of the intensity of the prevailing diathesis, more sure than can be derived from the examination of symptoms.

Several traces of a practice and doctrine similar to, or identical with, that of *Toleranza*, as taught by Rasori, are to be found in the writings of Dr Cullen. Reference has already been made to his free use of tartar emetic in fever attended with inflammatory diathesis. In a clinical lecture delivered in the session 1769-70, in discussing the management of a case of Fever, he observed—"With regard to this man, the only other thing remarkable is his bearing large doses of the tartar, and even from large doses having no stool. Men are of different sensibility with regard to Emetics and Purgatives, and this insensibility is very often increased in fever. Whether the tartar may operate in Fever without producing sensible effects elsewhere is uncertain; but, at least, we may suppose them less, and I hold it for a rule always to carry the dose to a sensible effect; and I regret when the stomach is irritable by a small dose, for then we have less effect on the fever. There is a particular case encouraging the large dose of tartar, and that is, when the intestines are less irritable; for I know no bad effects of tartar but excess of purging, in which case it does not prove critical, but serves to protract the fever. It is on this account that I proceed so cautiously in giving the smallest dose, till I know its tendency and operation; and when it operates little on the intestine, there are no bounds to the dose, for a little excess in vomiting is hardly to be feared." And, again,

in mentioning the case of a woman whose fever was attended with high delirium, and who was very little affected by Tartar Emetic,—Dr Cullen remarks,—“Most of our patients are affected to vomiting and purging by a grain or less. Here three grains had no sensible effect. This might be owing to peculiar constitution, *but it often arises from an insensibility produced by the disease.* Where we depend upon this medicine, the dose must be increased, but with caution.”

Dr Cullen has remarked the insensibility of the system in Maniacs to the action of Tartar Emetic and Opium.\*

Dr Cullen's attention had been strongly drawn to the influence of certain other diseases, besides fever, in requiring an increase of the quantity of medicinal agents which the system can bear without injury or with advantage, particularly the influence of Retrocedent Gout in producing tolerance of spirituous liquors, and the influence of Tetanus in producing tolerance of opium. In respect of the latter, he remarks, in his First Lines, § 1271,—“When the cure of the disease is to be attempted by medicine, experience has taught us that opium has often proved an effectual remedy; but that to render it such, it must be given in much larger quantities than have been employed in any other case; and, in these larger quantities, it may, in this disease, be given more safely than the body has been known to bear in any other condition. The practice has been to give the opium either in a solid or a liquid form, not in any very large dose at once, but in moderate doses, frequently repeated, at the interval of one, two, three, or more hours, as the violence of the symptoms seems to require. Even when large

\* Institutions of Medicine. Works by Thomson, Volume First, p. 132. CXXXI.

quantities have been given in this way, it appears that the opium does not operate here in the same manner as in most other cases ; for though it procure some remission of the spasm and pains, it hardly induces any sleep, or occasions that stupor, intoxication, or delirium, which it often does in other circumstances, when much smaller quantities only have been given."

In speaking of the employment of stimuli in the treatment of Retrocedent Gout, when of a threatening kind, and extremely violent, I have already adduced, at page 340, from his Lectures on the Practice of Physic, a passage containing the results of the experience of Dr Cullen.

The principle of the Endurance of certain medicines by the System, when in particular conditions, has, since the commencement of the present century, undergone at once expansion and modification. Besides the instances already referred to, this principle has been thought to be more or less generally recognized in the administration of tartarized Antimony for the treatment of Peripneumony, Bronchitis, and certain forms of Mental Derangement ; in the administration of Opium for Cholera, and *Delirium Tremens* ; in that of Mercury for Yellow Fever ; and in the use of the Sulphate of Quinine for Remittent Fever.

a. In certain forms of peripneumony, after blood-letting has been carried to a sufficient length, or where it is believed to be no longer safe, the employment of rather large doses of tartarized antimony has been found to be not only admissible, but requisite and beneficial. It is a remedy, the effects of which require to be carefully watched, that they may not proceed

to cause an excessive and dangerous degree of weakness. But all practical physicians agree, that, with caution, and where it is given with little fluid, it is in many instances followed by great benefit, and the eventual subsidence of the disorder. Upon this point, the observations of Laennec, in confirmation of those of Rasori, may be consulted.

b. In Mental Derangement, in which there are great excitement, restlessness, and some fury, the exhibition of tartar emetic in doses of two, three, or four grains, is often attended with speedy and effectual abatement of the symptoms. The same good effects have been witnessed from its use in Puerperal Mania, in which Gooch saw more than once a speedy cure result from its operation. In some of these cases, its use is followed by alvine dejections. But this seems not to be uniform, nor is it necessary to the therapeutic result.

The use of this mineral salt is not less necessary and beneficial in various cases of *Delirium Tremens*, more especially when this disorder is attended with great sleeplessness, restlessness, agitation, and sickness. The administration of two or three grains of tartar emetic is then followed by a state of depression and calm, which often terminates in spontaneous sleep and the disappearance of the distemper.

c. In the treatment of cholera during the last twenty-five years, Opium has been given in rather large doses, sometimes alone, sometimes with Calomel or Blue Pill, and sometimes with Ether or Chloroform, and, in all these circumstances, it has been believed that the system could endure, and required larger doses than in the state of health. Over this

idea, nevertheless, a certain degree of doubt has been thrown by those who maintain, that, on the one hand, the doses have been often hurtfully, even fatally large, and that, on the other, when patients recover after these large and repeated doses of opium, the symptoms of the peculiar affection of the brain, which has been so often witnessed in Europe, and which so often requires depletion from the head, and, nevertheless, terminates fatally, are the effects of excessive doses of this narcotic. It is by no means easy to prove that in cholera, opium may not have been given in hurtfully large doses; for in the wholesale and rather reckless administration of that drug, which generally takes place during the prevalence of that disease in the epidemic form, it is easy to see that the exact and just amount may often have been exceeded. But that these large doses cannot have been the sole cause of the subsequent cerebral affection, may be inferred from the fact, that the state of headache and stupor was most violent in patients in whom the stage of collapse was longest, and bore no proportion to the amount of the narcotic administered. The safe inference appears to be, that, though cholera may be considered as one of those morbid states of the economy, in which large doses of opium are both required and endured, yet it is by no means safe to trust to this remedy alone; and while its effects ought to be sedulously watched, it is of great moment that the physician should avail himself of the aid of auxiliary remedial means.

d. When Dr Sutton, in 1813, recommended, for the cure of *Delirium Tremens*, to the exclusion of the employment of bloodletting, the use of repeated

doses of opium, chiefly upon the ground that the disease is not one of an inflammatory character, it was generally allowed that the recommendation, with the prohibition of blood-letting, was a decided and valuable improvement in practical medicine. The practice was also found to be so generally successful, that physicians acted as if they thought that too much opium could not be given, while the patient was under the symptoms of the disease, and that no other therapeutic method or remedy was required. Continental physicians, especially those of Germany and Italy, rather shunned this exclusive method; and several of them, as Joseph Frank, Speranza, and some others, did not hesitate to employ blood-letting and purgatives in combating the symptoms of *Delirium Tremens*. The physicians of the United States, the cities and towns of which afforded extensive opportunities of treating the disorder, presented two opposite extremities; one party giving very large doses of opium, another party giving either none, or employing tartar-emetic, blood-letting, purgatives, and, in some instances, almost no treatment whatever, beyond confinement and the withdrawal of the cause of the disorder.

A considerable change in the opinions of physicians upon this subject has taken place during the last twelve or fifteen years. It has been found that not only are large doses of opium unnecessary, and in many instances hurtful, but that recoveries have taken place altogether without their use. In the course of my own practice, both private and at the Infirmary of this place, I have found that where it



was possible to administer emetics, nauseants, and purgatives, and by these means and the use of cold applications to the head, and warm fomentations to the lower extremities, to abate the violence and restlessness of the patient, sleep came on naturally, and opiates were unnecessary. The truth is, that so long as the irritation of the poisonous material acts upon the brain and spinal chord, the patient continues restless, delirious, and sleepless ; but as soon as this irritation is abated or withdrawn, sleep comes on spontaneously, as in the natural state. It is, further, a well-known fact, that, in certain circumstances, the symptoms of *Delirium Tremens* disappear spontaneously. It may be inferred, therefore, that though the guarded and cautious use of opiates may, in certain cases and stages of the disease, be useful, yet we must modify considerably that pathological principle, according to which it has been believed that the system can, under this morbid condition, endure large doses of opium ; and we may qualify, in a corresponding degree, the therapeutic principles upon which the treatment of the disease is most safely conducted, by admitting the previous employment of emetics, nauseants, and purgatives, in order to abate the violence of the characteristic symptoms.

e. During the prevalence of the great Yellow Fever epidemic of 1793, and between that event and the commencement of the present century, various practitioners thought that they had discovered, in mercurial preparations administered so as to affect the mouth, a certain cure for Yellow Fever and

Remittent Fever in their most intense forms ; and that if it was possible to give this mineral, so as to produce the characteristic effect on the mouth, the patient was safe.\* It was, accordingly, believed to be good practice to administer to patients labouring under Yellow Fever and intense Remittent, very large doses of calomel or blue pill, in some instances to the extent of 500, 1000, and 1600 grains;† and the periodical and other medical writings of England and the United States contain records of sundry instances in which mercury was given to a very large amount. It was in no long time found, however, that though certain cases, by some it is said many, recovered under this mercurial treatment, as it was named, yet patients suffering under Yellow Fever died after the gums had been completely affected;‡ and of those who recovered under the treatment, it was by competent observers doubted, whether they would not have recovered without the use of the mineral.

These experiments, nevertheless, showed, that when the system is under the influence of Yellow Fever or intense Remittent Fever, it becomes greatly less amenable to the action of mercury. The human frame in that state acquires a sort of physio-

\* Practical Observations on the Treatment of Acute Diseases ; particularly those of the West Indies. By William Wright, M.D., &c. Medical Facts and Observations, Volume VII., pp. 1, 5, and 7. London, 1797.

† A Sketch of the History and Cure of Febrile Diseases. By Robert Jackson, M.D., &c. Stockton and London, 1817. 8vo. Pp. 238, 241, 243.

‡ Ibid., p. 243.

logical insensibility to the action of the mineral, and perhaps to other agents. Whether it can be said to tolerate and endure this mercurial action better than in the state of health, is a point which may be left to the judgment of the reader.

*f.* The Sulphate of Quinine often requires to be given in very large doses ; and these doses to be often repeated and long continued, in order to subdue the symptoms of certain obstinate forms of Remittent or Endemic Fever. The effect of this use of Sulphate of Quinine is to induce ringing in the ears, and various symptoms of fulness and congestion in the head ; and as this assemblage of symptoms has been regarded by practitioners in the Tropics as indications of the physiological action of the remedy upon the system, it has been designated by the appropriate name of Cinchonism. It seems to be scarcely a matter of doubt, that this condition is one which is more readily endured or tolerated by the system, when under the action of endemic fever than either in the state of health or in other morbid states. At all events, very large quantities of the remedy have been given in such circumstances, until the economy is, as it were, charged with the substance.

Other medicinal substances, as Indigo and Iodine, have been given in such manner, and to such amount, as to try the powers of endurance in the economy. But enough has been said to elucidate the nature of this supposed capacity of the human frame for enduring the action of various powerful agents upon its organs. If we are to admit the existence of this power of endurance, readers will readily think of the

doctrine maintained by John Hunter of the incompatibility of two actions in the system at the same time, and may regard endurance as a modified illustration of this doctrine. Endurance or tolerance of powerful agents takes place chiefly during morbid conditions ; but this endurance or tolerance, when it exists and continues, either extinguishes the morbid state, or induces a condition of the system favourable to the abatement, the removal, and the abolition of that morbid state.

The account of the peculiar speculative opinions of Dr John Brown has unavoidably occupied a larger proportion of space and attention than that to which their intrinsic merits entitle them. From their relation to the doctrines of Cullen, and from the ingenious manner in which they were represented by their author to differ from these doctrines, it became indispensable to give some account of them. But a most erroneous estimate of the true nature of these speculations, and of their influence upon the Practice of Medicine among contemporaneous and succeeding physicians would be entertained, if the statements of the followers of Brown were to be received without question and without examination. In forming a just estimate of the nature and effects of the speculative opinions of Dr Brown, it must be observed, that, in Edinburgh, where their author lectured, and in England and Scotland in general, these opinions never made a strong or durable impression. They were listened to for a short time by a number of persons more remarkable for their love of novelty than

for soundness of judgment. They formed the subject of discussion, and not unfrequently of experiment, at the Medical Society of Edinburgh, and some other associations of students and young persons in general ; but they never exerted much influence upon the treatment of diseases as conducted in the hands of respectable English and Scottish physicians. Neither did the doctrines of John Brown engross, in the medical schools of England and Scotland, that amount of attention, or excite that degree of interest, which the representations of some foreign authors would lead us to believe they did. In Edinburgh, the interest excited by the doctrines of Dr Brown at no time penetrated into the University, and it was confined to one or two extra-academical teachers, more noted for paradox and singularity than for qualities supposed to distinguish rational instructors. In Glasgow and Aberdeen, these doctrines were scarcely known. In London, the lecturers attached to hospitals appear scarcely to have noticed these doctrines; and they certainly at no time became the subject of discussion or the foundation of practice. One or two surgeons, indeed, sometimes employed the terms Collapse, Excitement, Depression, Stimulation, and the analogous terms, Sedatives and Stimulants ; but this they had done previous to the promulgation of the doctrines of Brown ; and all the idea that they had of the author was, that he was fond of giving stimulants where other physicians condemned the employment of these articles, and of exhibiting opium in the treatment of symptoms which were in general more safely managed upon the antiphlogistic method. John

Brown himself was indeed almost the only lecturer in London, during the short time that he taught in the metropolis, who attempted to explain the theory and treatment of diseases upon the principles peculiar to himself. It was not, in short, to be expected, that in London, where the courses were at that time of only three months' duration, and where all the instructions were of a kind bearing directly upon practice and treatment, the speculations of Brown should occupy much or any attention; and there, accordingly, they never did excite more than temporary interest.\*

Other circumstances further contributed in a very direct manner to render the reception of the principles of John Brown, by the Medical Profession in Great Britain, not only partial and imperfect, but altogether null. At a period so early as 1731–32, the physicians and surgeons of Edinburgh had, under the auspices of the first Monro, Charles Alston, Robert Whytt, and Andrew Plummer, in that city, and Thomas Simson, and George Martin of St Andrews, formed themselves into an association for the purpose of collecting and preserving such isolated but important facts as individually could not be published with advantage, but, conjointly, might form a work of considerable value to the members of the Medical Profes-

\* “ It is neither in Scotland nor in England that the doctrine of Brown found most partizans. Italy received this doctrine with enthusiasm, and the progress which it made in that country forms in some sense a sort of prodigy. For if there is one country in Europe where the method of Brown must produce inconveniences, it is unquestionably Italy, where the susceptibility of the gastric system renders the practical application of this system in almost all instances dangerous.”—Broussais, *Examen des Doctrines Medicales*, Chapitre v., p. 150.

sion. Of these papers, the first volume appeared in 1733, from the press of Ruddimann ; and between that year and 1744, appeared other four or rather five volumes, the last being in two parts. The publication of these six volumes, several of the papers in which were contributed by practitioners in different towns in Scotland, some from practitioners in England, and some from Ireland,\* was most serviceable in two modes. They preserved and made known several important facts, as well as several instructive memoirs, which might otherwise have been lost, or left in a state of obscurity. They were at once an indication of the feeling of the Medical Profession in Scotland ; they gave to the Profession a decided taste for the collection of facts illustrating pathology and therapeutics ; and they imparted a disinclination proportionally strong to all speculations and hypotheses not resting on, and confirmed by, observations and experiments.

Though the labours of this Association received a serious interruption by the unsettled state of the capital of Scotland in 1745, yet, in a more general and comprehensive form, the Association, after the lapse of ten years, published, in the course of seventeen years, between 1754 and 1771, three other volumes, less exclusively devoted to medical subjects, but containing, notwithstanding, articles of great value.†

\* Medical Essays and Observations. Revised and Published by a Society in Edinburgh. Second Edition Corrected. Vol i., 1737 ; Vol. v., Part Second, 1744.

† Essays, Physical and Literary, in Three Volumes Edinburgh, 1754, 1756, 1771. 8vo.

Avowedly following the example of the first of these Associations, certain physicians in London, either connected with one of the hospitals, or otherwise enjoying good opportunities for observation and experience, began about, or previous to, the year 1750, to hold meetings for the communication of such facts as appeared to them to deserve being made known, and for mutual benefit in the practice of healing. Among the subjects considered, were not only remarkable examples of disease and the effects of remedies, but the reigning diseases of the season, with the methods of cure found to be most effectual; new discoveries in medicine at home or abroad; and especially the consideration of those therapeutic methods and remedies which the members themselves had subjected to trial. Among the members of this Association, William Hunter; Dr John Clephane, well known as the friend and correspondent of David Hume; Samuel Pye; George Macaulay; John Sylvester; George Cleghorn; John Fothergill; Thomas Dickson; Richard Brocklesby; in short, the principal physicians and surgeons of the metropolis held conspicuous places; and when their intention of publication was made known, their friends and correspondents enabled them in no long time to collect papers and essays, which were published in a first volume at London in 1757, under the title of **MEDICAL OBSERVATIONS AND INQUIRIES**.

Between this date and 1784, other five volumes were published, all containing a large amount of most important facts in Medical and Surgical Pathology and Therapeutics.



The MEDICAL OBSERVATIONS and INQUIRIES led to the publication of the Transactions of the College of Physicians in three volumes, between 1767 and 1785, and were followed by the two volumes of Medical Communications by the SOCIETY FOR PROMOTING MEDICAL AND SURGICAL KNOWLEDGE, between 1784 and 1790; six volumes of the TRANSACTIONS OF THE MEDICAL SOCIETY OF LONDON, between 1787 and 1805; eight volumes of MEDICAL FACTS AND OBSERVATIONS between 1791 and 1800; and three volumes of MEDICAL AND CHIRURGICAL TRANSACTIONS between the years 1793 and 1812.

These volumes, in number twenty-eight, which contain many important Essays and records of cases illustrating Medical and Surgical Pathology, Midwifery, Therapeutics, and Pharmacy, form at once an indication of the general character of the Medical mind in Great Britain, and reacted upon the mind of the profession, by giving the Members of it a desire to cultivate and improve the art of healing, by collecting facts and observations, and by admitting only such reasonings as were founded on well ascertained facts and well conducted observations. The publication of these volumes extended, it is to be remarked, over the last forty-three years of the eighteenth century, and over the first eight or ten years of the present century, and must, during that period, have created, if the taste was not already in active existence, a decided taste for information, founded on observation and experience, to the exclusion of speculative opinions and hypothetical dogmatism. It is not likely that the authors of the

papers contained in these volumes, who were living when the *ELEMENTA* of Brown made their appearance, gave to that work much attention. Nor is it more probable that the readers of such papers could repose confidence in the statements of a writer who had not established any character either for observation or experience in Practical Medicine. It is a matter of fact that they did not. The mind of the profession in England, indeed, was of a character too practical to be influenced by the work of John Brown. That work produced no effect either upon physicians or medical practice ; and even after it had been introduced, in 1795, in a corrected and amended English dress, with all the advantages resulting from the learning and judgment of Dr Beddoes, a commentator certainly friendly as well as benevolent, the work failed to produce more than a mere temporary effect, and is now consulted chiefly by those who are desirous to know the state of Medicine at that time.

It cannot now appear wonderful, we think, that the doctrines of John Brown never took a firm hold of the profession in these Islands.

In France, it has been already stated, the doctrines of Brown made an impression not stronger or more lasting. At the time at which they were made known in that country, Bichat was in the height of his reputation ; and as his physiological and pathological theories were more intelligible, more instructive, and certainly more clearly stated, it is not wonderful that the impression which they made were

exclusive of, and antagonistic to, those of the Scottish speculatist. In addition to this, however, Desault, Bichat, Corvisart, Portal, Pinel, Prost, Riche-  
rand, and various other eminent men, were devoting all their time and energies to the cultivation of Morbid Anatomy and Pathology, in the hope that, by combining Clinical Observation with Morbid Anatomy, they might most effectually improve and give efficiency and precision to the art of healing diseases. It was not, therefore, in France where such men as those now mentioned, were carefully studying the phenomena of diseases in the numerous hospitals of the cities of France, and tracing the effects of diseases in the dead body, that any attention was likely to be given to the obscure and fanciful speculations of John Brown.

The agreeable treatment, nevertheless, recommended by Dr Brown had been adopted by some practitioners more remarkable for credulity and confidence than for observation and judgment. According to the testimony both of Pinel and Broussais, traces of Brunonian practice were occasionally apparent both in civil and in military practice. What its effect was in the former, may be inferred from the testimony and practice of Pinel and Corvisart, Prost, Petit and Serres, and, at subsequent periods, those of Lerminier and Andral. That its effect on the army was most hurtful, we have the direct testimony of Broussais, who appears to have witnessed some most unequivocally bad results from its use, and who, accordingly, denounced the practice

in very decided and peremptory terms.\* In short, the practical application of the Brunonian doctrines appears not at any time to have been general among French practitioners ; and it is difficult to conceive how they could find supporters among men who devoted so much attention to Clinical Observation and to the practice of Morbid Anatomy.

It was chiefly, if not solely, in the medical schools of the German and Italian states that the speculations of Dr Brown excited interest, both considerable at the time, and much more permanent than in Great Britain. In making this admission, however, attention should be given to the kind of persons among whom this interest was greatest and of longest continuance. It was not among the most rational, the most learned, and the most philosophical of the teachers of these countries that the Brunonian doctrines found most favour and the most ready degree of acceptance. It was not among the assiduous hospital physicians that these doctrines were in highest esteem. We have seen that Rasori and Tommasini, indeed, and some of their followers, adopted some of the ideas of Brown, but modified them greatly in constructing and expounding the new Italian doctrine of stimulus and contro-stimulus ; and it is not doubtful that these speculatists were in some degree indebted to the hypothetical dogmas of Brown for

\* *Histoire des Phlegmasies Chroniques*. Paris, 1807 et 1813 *passim*.

*Examen des Doctrines Medicales et des Systèmes de Nosologie, &c.* Par F. J. E. Broussais. Deux Tomes. Paris, 1821. Tome Premier, Chapitre iv., pp. 58, 98–148.

several of the fancies which form part of their theory. But the great body of the most intelligent and best informed physicians and surgeons of Italy, for instance, Vacca, Scarpa, Monteggia, Palletta, Locatelli, Speranza, and Bufalini, either gave to the doctrines of Brown no attention whatever, or opposed them, or received them with many qualifications.

In Germany, the reception was a little different ; but so far as relates to the characters of the recipients, it was much the same. The doctrines were received and expounded for some time without modification ; and by some young and inexperienced writers they were defended rather strenuously. But those who showed these doctrines this degree of favour were by no means the first or the ablest members of the profession. Melchior Adam Weikard, by whom the doctrines of Brown were made known in Germany in 1795,\* by means of an indifferent translation from the Latin, was a man of little medical knowledge, despising all learning, bigoted, dogmatic, prejudiced, and is now remembered only as the translator of that work, and the editor or translator of various useless and forgotten writings. He was, indeed, a person of considerable enthusiasm, great activity, not a little conceit, rather violent passions, little judgment, and about as little taste.

An illustration, not void of significance of the

\* JOHANN BROWN'S Grundsätze der Arzneylehre aus dem Lateinischen übersetzt. Von M. A. Weikard. Frankfurt, 1795. 8vo.

mode of reception of the doctrines of John Brown by the German mind is afforded by the history of the reception respectively accorded to these doctrines by John Peter Frank and his son Joseph Frank. John Peter Frank, German by birth, and employed in various German towns from 1767 to 1784,\* but in 1785 appointed to the office of Clinical Teacher in the Medical School of Pavia, was, after teaching there for ten years, called by the Emperor, Francis the Second, on matters connected with Military Medicine, to Vienna, in the beginning of 1795. In the meantime, his son Joseph Frank was, by the Local Government of Lombardy, appointed to perform the duties previously exercised by his father. Joseph Frank, then a young man, not more than twenty-two years of age, had become acquainted with the doctrines of John Brown, apparently by the perusal of an Edinburgh edition of Brown's Elements, brought from Scotland by Onufrio Scassi, a young Genoese; and he had evinced his attachment to these doctrines, by publishing a translation of the work of Dr Jones, which he had illustrated by notes. It appears that previous to the year 1795, neither the work of Brown nor that of his supposed disciple Jones, had been much to the taste of John Peter Frank, who was certainly disposed either to question the justice and the originality of the doctrines, or at least to suspend his judgment. Not so with the son

\* In 1767, Public Physician at the Thermal Waters of Baden; in 1769 to 1772, Court Physician at Rastadt; then Town and District Physician at Bruchsal, in the Bishoprick of Spire; and, in 1784, Clinical Physician at Goettingen.

Joseph, who, in some degree, captivated by the instructions of Brown on the administration of Opium in the advanced and debilitated stage of Fevers, and the use of Camphor, Musk, Sulphuric Ether, especially in Nervous Fevers, showed himself rather a strong partizan and adherent of the Scottish innovator. This tendency was once more evinced in the Clinical Report of the Pavia School for 1795, published at Pavia in 1796. In truth, Joseph Frank partook of the haste and precipitancy of most of the young medical persons in the Italian Schools ; and though he was probably not so violently affected by this passion for doctrines apparently new, as some of the other young Italian physicians, it was manifest that he had not entirely escaped the influence of the infection.

John Peter Frank, the father, appears to have been satisfied that Joseph Frank, the son, had gone quite as far in adopting the doctrines of Brown as it was possible to do, and much farther than was proper and prudent. For he lost no time in prefixing to the Clinical Report of the son a pretty ample introduction, in which he disavowed, as distinctly as it was possible to do, the charge to which some thought he was liable, of being a follower of Brown, and of having adopted the peculiar doctrines of that writer. He allowed that the work of Brown contains several things deserving to be known and remembered, but many things so questionable, that a cautious physician ought to pause and think before he gives them credence.

He professes that he is neither to pronounce a panegyric upon the work of his son, neither to adopt

and defend the foreign theory which that son has partly embraced, nor to attack it. From early life, he had always been averse to hypotheses and controversies thence proceeding; and though in the duty of public instruction, he both proposed his own views, and passed not over those of others, yet he was rather inclined to inculcate doubt on many subjects involving mere opinions.

He states that the harshness of the style, and the obscurity pervading the work of John Brown, formed impediments at first to its being understood; and even after some of its difficulties were explained, there were several who were deterred from studying the *ELEMENTA* by the deviation of the author from the right path of truth and the clear language of experience.\* Coming, however, at a time when the humoral pathology was still rather prevalent, and presenting a system founded most completely on the doctrine of the simple and living solids, the *ELEMENTS* were received with a degree of attention and interest, which no work previous to that time had been able to command.

It is remarkable, that, in this representation, John Peter Frank shews that he was quite unaware that the peculiar characters of this Physiology and Pathology of the Solids had been fully expounded by William Cullen twenty years before this time. Indeed it is perfectly clear, from the language of Frank the

\* *Ratio Instituti Clinici Ticinensis. A Mense Januario usque ad finem Junii Anni mdcxcv. Quam reddidit Josephus Frank, M.D., &c. Præfatus est Joannes Peter Frank, p. 23 et 24. Venetiis, 1799. Part I. et II.*



elder, that the *Elements* of Brown were chiefly useful in making known to the Italian physicians the doctrines of Cullen ; and had these physicians read with any attention the already published writings of Cullen, they would at once have perceived the true source of the *ELEMENTA* of Brown, and they would have had less difficulty in comprehending the doctrines proposed by this writer. But it has been remarked in a former part of this volume (p. 355), that a partial and unjust review of the doctrines of Cullen had been given by Vacca Berlinghieri in 1787 ; and the influence of this review was sufficient to bias the physicians of Italy against the real services rendered by Cullen to the science and art of Medicine, and to prevent them from studying his writings.

Harshness of language, and obscurity of style, mostly proceeding from affected brevity, were not the only objections which John Peter Frank urged against the *ELEMENTA* of Brown. The mode of expression was stronger and more violent than becomes a scientific work, in which those things which are true should be enunciated, indeed, with firmness and dignity, and in such manner that truth should strike error only, not the erring person, and deter from embracing it. The tone in which Brown expressed his opinion upon matters very obscure, and, at the same time, very serious, which was more confident and decided than was becoming, was another objection.\* Often he cuts, and does not untie the knot ;

\* In ipso Brunone dictionis quam in aliis laudare non potui, vehementia mihi displicet. . . . Displicet sermonis, etsi plerumque perbrevis, hinc inde tamen verbosi (§ 167, 168),

and though by no means informed by experience on medical subjects, yet he composes and delivers axioms with reckless confidence. It is not uncommon, Frank continues, for English writers who are ignorant enough of the languages and of those matters which have been established by other nations, to regard as new every thing which they adduce. This failing, also, is common to Dr Brown, namely, that he expounds as new several principles which are not new, and he seems to accuse all persons of a degree of gross ignorance, in which they certainly are not.

As to his comprehending IRRITABILITY and SENSIBILITY under the single term and property of EXCITABILITY, and representing this alone to be diffused through the whole system of the animal body, though over this point there hang not few doubts; yet, uncertain of the truth, Frank allows that so it may be. But why the whole science of Medicine should turn upon these cardinal points and that of *stimulus*, is a matter which he does not sufficiently understand. It is unnecessary to follow the Austrian professor in his subsequent meditations and criticism on the ELEMENTA. It is sufficient to say that he disapproves of the twofold division of Debility, and adds that there is a third, namely, *ap-asperitas*, *obscuritas*; qua factum est, ut ipsos BRUNONIS inter discipulos diversa doctrinæ interpretatio, ac non exigua opinionum discrepantia exortæ sint. Displicet major ac in rebus tam abditis simulque tam seriis asserendis esse debent, persuasio et securitas. Conscindit saepe nodum, non solvit auctor systematis; ac rerum etsi medicarum experientia haud maxime instructus, axiomata tamen condit, suisque tutus offert. Ratio Instituti Clinici Ticinensis, Præfatio, p. 39.

*parent* Debility, which Brown has altogether omitted, and which yet is by no means rarely witnessed at the couches of the sick. Frank takes notice of various errors in point of fact committed by Brown as to the symptoms of *Plethora*; the shivering which ushers in inflammatory diseases; those relating to Soporose and Comatose Diseases; and, finally, the division into the two great families of Sthenic and Asthenic Diseases; of which last he justly remarks that it is a distinction which is contradicted by daily practice, and which it would be extremely unsafe to trust to.\*

This preface was intended as an answer to those who represented John Peter Frank as an advocate and adherent of the principles of John Brown. It is, in short, the clear and explicit disavowal of an experienced, an aged, and a cautious physician, and, as such, it was doubtless received by the small number who took time to read it. It was a warning to the junior members of the profession against being led away by specious hypotheses and dangerous innovations. Apparently its effect was not altogether lost upon the son Joseph Frank, who lived long enough, if not formally to abjure his first precipitate adoption of the Brunonian Doctrines, yet to allow these quietly to settle into that state of insignificance and neglect, which eventually awaits all speculations not resting on the solid foundation of observation and experience. In his excellent and elaborate work on the Practice of Medicine, published

\* *Præfatio*, 58, 59.

at Turin between 1821 and 1825,\* the name and system of John Brown are merely mentioned ;† but the peculiar doctrines and writings of the author find no place either for commendation or adoption.

The reception of the doctrines of John Brown by such physicians as Hufeland and Valentine Ernest Von Hildenbrand, was similar to that accorded by John Peter Frank, rather more decidedly hostile. Hufeland, indeed, appears never to have given the doctrines much attention, and Hildenbrand scarcely deigns to mention them, except to point out their dangerous tendency. It is further remarkable that Reil, who was in the height of his activity at the time when the Brunonian doctrines were made known in Germany, scarcely ever mentions Brown, though he employs the term *Excitability*, and has frequent occasion, in his different writings,‡ to treat of the properties and functions of the Nervous System, and to speak of various states in which it is liable to be placed both in health and in disease. *Lastly*, Kurt Sprengel, in his History of Medicine, criticised at considerable length, and with great justice, the doctrines of Brown ; and the result was, that he exposed clearly the great imperfections of the system.

\* *Praxeos Medicæ Universæ Præcepta* Auctore Josepho Frank. Partes Duæ, Volumina undecim. Editio Secunda. Turini, 1821, 1825.

† *Praxeos Medicæ Universæ, &c.* Volume I., p. 25. Turini, 1821.

‡ Johann Christian Reil, U. S. W. Ueber die Erkenntniss und Cur der Fieber. Allgemeine Fieberlehre. Besondere Fieberlehre. Vier Band. Halle, 1799–1805.

And *Archiv Fur Anatomie und Physiologie*, Band. II., *passim*.

After enumerating these, he concludes with the following inference. "We see by this that Brown was not acquainted with diseases, and that he has wittingly put an erroneous interpretation upon the doctrines of his great master, William Cullen, in order to establish a new one of his own."\*

Whether it were owing to the influence and example of the writers now mentioned, or to other causes, the Schools of the German States continued a much shorter time under this delusion, and recovered from it at a much earlier period, than those of the Italian States. Though the German mind is prone to indulge in fanciful speculations, and though several German writers undertook to expound and to defend and apply the doctrines of Brown, yet, in the course of a few years, many of these productions were forgotten and neglected; and the theory of the Scottish speculatist was quoted rather to shew its insufficiency than to recommend its employment as a guide in investigating the nature and conducting the treatment of diseases.

When we inquire into the circumstances which contributed to the different reception which was accorded to the doctrines of Brown in different countries, it is not by any means easy to discover what these were. The following facts and considerations, nevertheless, are not undeserving of attention.

1. Most of the Italian physicians, and perhaps some of the German physicians, were altogether ignorant of the fact that Dr Brown had derived most, if not all, his importance, from the circumstance of

\* Geschichte der Medezin. Von Kurt Sprengel, Band vi.

having been resident in the family of Dr Cullen as instructor to his children, and occasionally as amanuensis to the Professor himself, and believed, and were led to understand, that he was a professor in the University of Edinburgh. This was the belief of Rosario Scuderi, the learned author\* of the History of Medicine, Ancient and Modern; the same was the belief of Guani,† and, not less, of Tomma-

\* "In the same University of Edinburgh, and in sight of the brilliant successes of the physiological theory of CULLEN, BROWN, his colleague and rival, left no means untried to put forward and substitute for the first a different system. This appears not at first to have seriously interested the attention of physicians, and to have been regarded hitherto as a shapeless production of a fertile and heated fancy. But the author being irritated by the unfavourable reception which he at first met, and looking with a gloomy and jealous eye on the constantly increasing progress of his adversary, applied himself with so much ardour to embellish his system, and to strengthen it by new arguments, that he succeeded at length in presenting it under an aspect less irregular and more attractive. Nor was the author deficient in talents; being endowed with an acute and penetrating genius, with an austere and vigorous judgment, and with a certain force and depth of reasoning, of which the history of Medicine furnishes few examples. A sufficient train of young persons, dazzled by the seducing air of simplicity and of regularity, attached themselves to this system, and undertook its defence. The number of partizans increased, and Brown beheld himself the Head of a new sect, which incessantly numbers some adherents both in Great Britain and elsewhere."

Introduzione alla Storia Della Medecina Antica e Moderna. Di Rosario Scuderi, M.F. Raccolta di Opere Mediche Moderne Italiane. Tomo Quarto, p. 259. Bologna, 1827. 8vo.

† "Brillava ancora in tutta la sua luce quella di Cullen, che un altro Riformatore suo collega e rivale nella stessa Università, venne clamorosamente a comparir sulla scena." Del

sini;\* and while it is manifest that these were not the only Italian physicians who entertained this idea, it is not doubtful that this error, when published, was adopted without question. By some it may be argued that this mistake, admitting its occurrence, could not avail in recommending the doctrines of Brown to the Italian physicians; and that these doctrines must have been canvassed, and received or rejected on their intrinsic merits alone, without reference to the position and rank of the author. It cannot be denied, that the latter is the just course, and that if the *ELEMENTS* of John Brown really contained sound, true, and natural pathological principles and serviceable therapeutic instructions, they were entitled to be adopted, whether the author were professor or not; and they merited all the attention which they received from the Italian physicians, and the discussion to which they gave rise could be the effect only of a certain amount of genuine intrinsic merit. Admitting the justice of these inferences, it is not less true, that the circumstance which so forcibly recommended the

*Controstimolo. Introduzione. Raccolta, Tomo Quinto, p. 78. Bologna, 1827.*

\* "If it be true, as I at least think, that Hoffmann, Baglivi, and Cullen, illuminated in a certain degree the path which Brown had the merit of first opening, it may be in like manner maintained, that, without the *New Elements* of the Professor of Edinburgh, the fundamental principles of the modern doctrine would not have been discovered, and the most useful ideas of the modern reform would not even have been conceived." *Della Nuova Dottrina Medica Italiana Prolusione. Bologna, 1817. 8vo. Notazione iv., p. 50.*

doctrines of Brown in the Italian Peninsula, was the belief that he was a popular and celebrated professor in the University of Edinburgh, the rival and colleague, as it was imagined, of Cullen. Never, perhaps, did the maxim of *omne ignotum pro magnifico* receive so strong and so pointed an illustration. We have seen that the Elements and their aphorisms were regarded as composed in rugged unpolished language; that they were so obscure, that they required multiplied explanatory notes and illustrations; and that even then, they were not in all points intelligible. The Pathological principles were often mere assumptions, not proved by facts or evidence of any satisfactory description; and the therapeutic directions, especially in reference to such diseases as Apoplexy, Palsy, Epilepsy, Hysteria, and various other affections of the Nervous System, were chiefly remarkable for differing from the ordinary established practice, and for the dangerous tendency which the practice founded on them has been since proved to possess. Even in Gout, in which Dr Brown maintained his great and constant success, his treatment was a lamentable failure; and if no other instance of its bad effects existed, the case of the author himself ought to have been a striking memorial of the pernicious consequences of the treatment which he recommended.

Finally, judging from internal evidence, we think it not unjust to say, that the ELEMENTA were the production of a mind vigorous, probably, and ambitious of originality, rather fond of paradox than truly independent, but coarse, without taste or delicacy,



totally unrefined by the previous training to which it had been subjected, and quite indifferent as to the accuracy and truth of the statements which the author made.\*

With all these unfavourable circumstances, nevertheless, in the intrinsic character of the *ELEMENTS*, the work was received by many of the physicians of Italy, especially the young and inexperienced, in such a manner as shewed the deep interest which they took in it. It excited discussions and controversies, such as no other single performance had ever previously done; and it at length was made the foundation of a system of Medical Doctrine, which, for fifteen or sixteen years, exercised a powerful sway over the Medical Mind of Italy. It was in this instance distance that lent enchantment to the view.

2. Connected with the circumstance already mentioned, was another which tended to invest the character of Dr Brown with a considerable amount of influence and weight. This was the circumstance of his acting as private medical teacher to the medical students† preparing for graduation, and, even-

\* For examples of this, see Notes ccxxviii., z, p. 233; ccxxix., a, ccxl., e, page 274, Beddoes' Edition.

In some of these notes, the memory of the author is at fault, and he makes incorrect quotations; as, for instance, when he states that Pythagoras had, upon discovering the forty-seventh proposition of the First Book of Euclid, jumped about in an ecstasy, and exclaimed, "Εὐρηκα." It was not Pythagoras who uttered this exclamation, but Archimedes, when he discovered the method of detecting to what extent Hiero's crown had been adulterated by copper by the fraudulent goldsmith.

† It has been asserted by Dr Beddoes, that Mr John Brown

tually, as a Lecturer. Foreign nations cannot form any idea of a lecturer on any branch of science apart from a University, a Medical School authorized by the Local Government, or some establishment under the sanction of the Government.

In Continental countries, Governments perform all functions, and especially take charge of institutions for public instruction. In Great Britain, on the other hand, Government leaves a great deal to be done by individuals, by voluntary associations of men, and by private enterprise. The position and character of such public Lecturers and Teachers as have long been common in London and Edinburgh, are to the continental understanding, matters of which they form a most inadequate idea. The consequence is, that of every one who is a lecturer, who delivers lectures in public, they form the idea that he is a professor, and is in some way connected with a University, or is authorized by the authorities to hold the position which he has assumed. It is not unlikely that to this idea they are partly led by likening the public and private Lecturers of London and Edinburgh to the *Aggrégés* or Associates of the Foreign Universities. In the instance of John Brown, it is not to be doubted that the circumstance

was permitted by Dr Cullen, with the aid of Dr Cullen's Notes, to repeat in the evening the Lecture of the day. This statement, however, rests on no solid foundation, and by those who had best means of knowing, has been generally regarded as entirely fictitious. Mr Brown was, in truth, at the time specified, 1763-1764, down to 1776, what is vulgarly called a Medical Grinder;—a most useful character, but not a Lecturer. See Appendix, Note M.

of his giving instructions to many of the medical students preparing for examination, led many at a distance to suppose that he was a professor ; and when he subsequently commenced to lecture on Medicine, the fact of his prelections being attended and relished by many zealous and attached pupils, contributed unquestionably to magnify his merits and character, and to make him appear at a distance as a great and brilliant luminary in Medical Science. It can scarcely be matter of doubt, that, in Germany and Italy, the mere circumstance of Dr Brown expounding his doctrines in public to admiring, if not intelligent auditors, contributed to invest these doctrines with a large proportion of that interest and importance which, in the eyes of foreign physicians, they for some time possessed.

3. Another circumstance which must have exercised considerable influence in procuring attention to the doctrines of Dr Brown is the fact, that Tommasini, and probably other Italian physicians, were in great error as to the comparative priority of the publication of the respective writings of Cullen and of Brown. It was the confident belief of Tommasini, in the outset of his career, that the publication of the *ELEMENTA* and other writings of Brown had preceded the publication of the doctrines peculiar to Cullen ; and this mistake he had not corrected at so late a period as 1820, when he published the First Part of his Treatise on Inflammation and Continued Fever. In that work he not only considers the doctrines of Brown upon Inflammation before he examines those entertained by Cullen, but he states that

the Elements of Practical Medicine (First Lines of Practice of Physic) by the celebrated William Cullen were published in 1783, subsequently that is, he adds, to the ELEMENTA of Brown and to the Illustrations by Robert Jones.\*

In ordinary circumstances, it would be of little moment whether the First Lines were published a few years earlier or later than the ELEMENTA ; for, granting the assumption that they were so published, other circumstances, well known, and presently to be mentioned, would prevent every impartial judge from assigning the merit of originality to Dr John Brown. As, however, Signor Tommasini has attached to this erroneous statement a high degree of importance, and as the statement involves a serious and decided error, and is at variance with fact, it is quite reasonable that the truth should be made known and the error rectified.

It is unquestionably true, that the first complete edition of the First Lines, in Four volumes octavo, was published in 1783, and that the ELEMENTA of Brown were first published in the year 1780. But it is not less true, that the First Volume of the First Lines, which contained the doctrine of Fever and Inflammation, and many of the pathological doctrines peculiar to Cullen, was published in 1776–1777, three years before the ELEMENTA made their appearance ; that the second volume, containing the

\* Della Infiammazione e Della Febbre Continua Considerazioni Patologico-Pratiche. Di G. Tommasini, Professore di Clinica Medica Nella P. Università di Bologna, &c. Pisa, 1820. 8vo. P. 51 and 52, and Note foot of page 52.

**EXANTHEMATA** and **HEMORRHAGIES**, appeared in 1779, one year at least before the publication of the **ELEMENTA**; that the third volume was published in 1783, and the fourth alone was delayed till 1784. Of the First Volume of the First Lines, which was regarded as the most remarkable medical work of the time, a Latin translation was published at Antwerp in the year 1779; and the peculiar merits of the work were made known on the Continent, partly by this translation, and partly by an account of the book in one of the French Transactions.\*

Independently, however, of the facts now stated, the Institutions of Medicine, in which were delivered all the doctrines peculiar to Cullen regarding the Living Solid, the Nervous System, and its Functions, Excitement, Collapse, Sensibility, Irritability, in short, all the doctrines by which his physiological opinions and medical practice were distinguished from those of other teachers, were published first in 1772 and again in 1777; so that the whole of the doctrines expounded in this performance were before the students and the profession from three to eight years previous to the date at which the *Elementa* of Brown made their appearance.

It must be further observed upon this topic, that several, if not all, of the physiological and pathological doctrines peculiar to Cullen, and all the therapeutic deductions founded on these doctrines, had been made more or less fully known to the profession both in this country and on the Continent, though

\* See this Volume of Life, page 100.

not in a very perfect shape, by the clandestine publication, in 1773, of the Lectures on *Materia Medica* which Dr Cullen had delivered in the session of 1761.

All these circumstances, however, though well known and admitted, form very feeble grounds for maintaining the claim of priority and originality in favour of Dr Cullen, when compared with the known facts of the mode in which Dr Brown came to offer himself before the public as an authority in medicine. From the year 1760 down to that of 1770, embracing a period of ten years, Mr John Brown had been attending the medical classes of the University of Edinburgh, particularly the lectures of Dr Cullen on *Materia Medica*, in 1761, those on chemistry in 1761–1762 and the two subsequent sessions, those on Institutions of Medicine and Clinical Medicine for different seasons down to 1773, and unquestionably those on the Practice of Medicine after 1776.

It seems to be so absurd to imagine, that one who was in the position of a Medical Student for ten or eleven years, and most of whose available time was spent either as tutor to young persons at school, or, as afterwards, as private teacher to medical students preparing to graduate, could be so perfectly acquainted with medicine as to be thought to precede the individual from whose instructions he had derived most of his knowledge, that, to us who know the facts, it appears altogether extraordinary that so groundless and erroneous a statement could be made. All the characteristic doctrines of Cullen were well known in Edinburgh, and even in Loudon, previous to 1780, and probably to no one better than to Mr

John Brown ; whereas the author of the *ELEMENTA*, which were published in that year, had been nearly to that time occupied in obtaining, from the Lectures of Dr Cullen, that information which he was now to place before the public in a form apparently new and unknown.

From these facts, therefore, it follows that the assertions made by Tommasini are merely the result of ignorance, carelessness, and inattention. These assertions, nevertheless, must have exerted considerable effect in Italy and among Italians in making it be believed, that Brown was a more original genius than Cullen, and that all the medical doctrines which he proposed were the offspring of his own intellect ; while the truth is, that all of them were either borrowed from Dr Cullen, or were the doctrines of Cullen modified, perverted, and mystified, in order to give Brown a sort of right of property in the doctrines so treated.

With all these explanations and allowances, nevertheless, it will always be regarded as a remarkable psychological phenomenon, that a work so imperfect, so hypothetical, containing so little of really useful information, and so many erroneous and fanciful notions, was capable of producing, among a people so learned as the Italians in general are, so great and deep interest, so much attention, and of giving rise to discussions and controversies so animated and so long continued. It is, indeed, a curious chapter, not only in the history of Medicine, but in the history of Mental Philosophy.

A sight still more extraordinary was yet to be

witnessed. It was that of a European Government assuming to itself the power of deciding upon the merits of a system of Medical Theory and Practice. In the year 1796, the Council of Health of the Austrian Armies published an Ordinance, entitled "Instructions respecting an Improved Method of Treating Diseases, and particularly Nervous Fevers, for the use of the Austrian Physicians in Italy." In this document, as Sprengel informs us, the great mortality which had prevailed up to that time among the troops, was attributed to the effects of the debilitating, evacuant, and lowering treatment which had been employed ; and the attention of the army physicians and surgeons was directed to the Brunonian division of Debility into Direct and Indirect, while it was enjoined upon them as a duty to have recourse to the stimulant and strengthening method of treating the diseases of the soldiers. It is not wonderful that this proceeding was disagreeable to the opponents of the new doctrine, as well as to those who were merely indifferent to it. One of the manifest results was, that a military physician of Austria published upon the Ordinance a critical examination, in which he not only shewed the impropriety of issuing such an order, but even insinuated that the practice was enjoined and recommended in order to relieve the Government, from the burden of pensions, under which it was supposed at this time to be suffering.\*

It is not possible to believe that any government could be so void of principle as to be actuated by

\* Kurt Sprengel, Versuch Einer Pragmatische Geschichte der Medizin. Band vi., Sect. xvii., Kap. viii.



motives so greatly to be reprobated. The measure appears to have been simply one of those imprudent and injudicious proceedings which governments of all kinds occasionally commit ; and though it may to us appear strange for any administration to interfere in the details of the methods of treating the diseases of their troops, it is not more strange than might be expected of an administration so officious and so fond of intermeddling in every thing relating to its subjects as the Austrian Government have at all times shewn themselves capable of being. The ordinance of 1796 was simply a blunder, an instance of extreme government ; and such it shewed itself in no long time to have been. It is not at all improbable that it was occasioned by the secret influence of some zealous and indiscreet physician who was more listened to by the Austrian ministers than he was entitled to have been.

To those who know what were the qualifications of Dr Brown for the duty which he undertook, all this must appear extraordinary indeed ; but the ultimate neglect and oblivion into which his system fell, will appear perfectly natural. No one of just feelings or generous mind would underrate the useful duty of tutor, or depreciate learning though in the humble character of a transcriber. But the truth must be told ; and the effects of the true position of Dr Brown should be fairly stated and justly estimated. It is needless to say that Dr Brown was not only not a professor. He was not even a moderately well-prepared private Lecturer. Beyond a good knowledge of the language of Ancient Rome,

he had no qualification for the character of assuming the duty of public Instructor in Medical Science. Language is only the key to knowledge, and unless it be judiciously and industriously used, the possessor may be comparatively ignorant. The circumstance that John Brown had spent most of his time in the occupations now specified,\* shews that he had not studied disease in a practical manner. Good Physicians and useful Lecturers are not made by the mere knowledge of one dead language, however numerous and excellent may be the writings in that language. It is in dissecting rooms, in hospitals, and in situations where the effects of disease can be studied in the dead body, and compared with the symptoms presented during life, that physicians acquire that knowledge which is to make them useful members of their profession, skilful in recognising the presence and true nature of disease, efficient practitioners, and safe instructors of their younger brethren. Unless, therefore, we can suppose that physiologists and physicians are made by mere intuition, and that their fitness for the practical duties of their profession is in the inverse ratio of their experience, and the time spent in acquiring a knowledge of the characters of disease, and the best methods of treating it, it is evident that Dr Brown neither possessed nor sought opportunities of acquiring that kind of knowledge, which is of all the most necessary and important to the physician.

Dr Cullen had recommended to his pupil to study practical anatomy to such a degree as to enable him to give demonstrations ; and probably the discipline

\* See pages 474, 476.

which this study could not fail to impart to the mind, might have acted as a counterpoise to the tendency to speculate, and the love of hypothesis which Brown had evinced. Brown followed this advice, not to the extent recommended by Cullen, but only to that of preventing himself from being quite without anatomical knowledge. The mind of Brown, indeed, appears at no time to have been capable of steady, persevering, and systematic application. He had embarked in the business of teaching without that amount of information which alone can make teaching efficient and useful; his knowledge, limited in extent, was desultory, loose, void of precision, void of methodical order; assumptions instead of carefully ascertained facts, specious hypotheses instead of patient generalizations; these were the means by which John Brown proposed to reform medicine as a science and as an art;—a noble object certainly, had the means been adequate to the end. But, as if to render this inadequacy still more detrimental, he had acquired habits of convivial indulgence and intemperance, which were equally fatal to the progressive advancement and precision of his own knowledge, and the faculty of rendering that knowledge useful to others. Over this mournful part of the history of John Brown, however, charity obliges us to draw a veil; and we quit the subject with the single remark, that whoever wishes to be useful to mankind, either as a physician or an instructor in the art of healing, should, above all things, practise temperance and self-denial, both for his own sake and as an example to others. If in either

character, or both, he is successful, he may be assured that much of that success is due to the exercise of these useful qualities. If he is not, and no man can always command fortune, he has at least the consolation of knowing that the want of successful results is not owing to his own conduct.

By those who have appeared as the eulogists of the intellectual superiority of John Brown, the defects now referred to have been represented to be the only moral blemishes which tarnished the brilliancy of his genius ; and a considerable degree of indulgence has been solicited in behalf of one whose mental endowments enabled him, with such failings, to create so strong a sensation in the profession. This idea is altogether a delusion,—an assumption which is contradicted by the whole course of the latter ten years of his life, when, if at any time, he might be expected deliberately and without passion to regulate his conduct. It was at this period of his life, when he had attained his forty-third or forty-fourth year, that his proceedings are marked by complete want of candour and sincerity, and by ingratitude to the man to whom he was indebted for all the distinction which he possessed. The foregoing sketch of the doctrines of Brown and their consequences has been written to little purpose, if it has not thence appeared, that Brown was quite cognizant of the fact, that, in promulgating the doctrines contained in the *ELEMENTA*, and the system which he pronounced to be his own, he was reproducing under very transparent, yet cunningly devised, modifications, certain of the doctrines of Cullen ; that in practical applica-

tions, where he agreed with that teacher, he was right, but not original : and that where he differed, he was either anticipated by others, or had wandered into grievous practical errors. It would be ascribing to Brown a degree of obtuseness, of which he nowhere gives proof, to suppose him to be ignorant of this ; and in contemplating his proceedings during the latter nine or ten years of his life, it is altogether impossible to screen him from the imputation of want of candour, in producing, as his own, what he could not fail to know belonged to another, and what common justice, as well as independence of spirit, ought to have made him acknowledge, in terms so clear and so ample, that no room for doubt could be left. True genius is above claiming that which belongs not to it ; and if Brown possessed genius, he was certainly altogether void of this attribute of the quality. It argues a very strange degree of moral obliquity to see a man going on for years in asserting his right to principles, to doctrines, and to facts, which he has learned from another, and actually quitting this world in the belief, that because he had expounded these doctrines in a dead language, and in a style and manner peculiar to himself, he was the true author of the whole. Yet was this done by John Brown. And who was the person, it may be asked, towards whom he thus acted ; whose just title he not only failed to recognise, but whom he used every means to deprive of any merit that might belong to those services which all his contemporaries, and, with few exceptions, all his successors, agreed that he had rendered to the science and art of medicine ? It was

his instructor, his benefactor ; and the painful task of recording this omission on the part of Brown has been imposed on the impartial historian of these transactions.

Notwithstanding, however, all the mental ferment to which, in Italy, the introduction of the doctrines of Brown had given rise,—notwithstanding the domination which, either by itself or in its modified form, this system of doctrines exercised over the minds of Italian physicians ; the time was approaching when the system was to be reduced to its just dimensions, and when it was to be beheld in a more natural light, divested of that false glare by which it had been hitherto surrounded. To this change various circumstances concurred ; but the subversion would not have been complete, had not one of those hands which had been most active in raising the system, and invoking in its favour the feelings of his countrymen, been also energetic and decided in contributing to its downfall.

In the year 1799, the course of war rendered the Republic of Genoa an object of contest and acquisition between the French Republic and the Austrian Government. The French were already in possession of the city and territory ; while the Austrians were anxious to gain possession of both. This they would have accomplished, in consequence chiefly of the great diminution of the French force which held the city, had not Massena, with a large reinforcement, checked the progress of the Austrian troops, until, in the close of March 1800, he was obliged to

retire within the city. The city was besieged by land and blockaded by sea ; and though a quantity of wheat and other provisions had been previously thrown into it, this was only sufficient to rescue the garrison and the inhabitants from absolute famine. The privations of the Genoese became extreme ; so dreadful, it is said, that they were unequalled in any siege of modern times. The wretched inhabitants were driven to satisfy the cravings of hunger by eating the grass and most common herbs that grew by the sides of the streets and roads. Fever, which had appeared in August 1799, increased in April 1800, and affected immense numbers of the inhabitants of Genoa, as well as of those who, in the vicissitudes of the time, had taken refuge within its walls.

Rasori who was in Genoa in June 1799, had soon occasion to observe that the atmosphere was more irritating than that to which he had been accustomed in the plains of Lombardy. A sense of heat in the windpipe, and cough, which he attributed to this irritation, led him to observe a sober, temperate life ; to eschew animal food, tea, coffee, and especially vinous liquors, and live mostly on vegetable articles. The same phenomena of tracheal irritation he remarked in other Piedmontese and Lombards.

The cases of fever began to appear in small number in August 1799, and continued throughout the autumn and winter. Though not numerous, they presented the same characters. In the spring of 1800, that is, in April, they increased, and became very numerous in summer. In the beginning of

July 1800, Rasori left Genoa for his native country ; and at this time they were most numerous and destructive.

The population of Genoa was supposed at the time mentioned to have been on the verge of 80,000, if not above that number. It is calculated that the ordinary weekly mortality was from 92 to 100 persons. In the first week of April ending on the 5th of that month, the mortality rose at once to 196, and by the last week of April it was 218. In the end of May the deaths rose to 382 in the week ; they increased during the month of June, being in the last week 508, and in the second week of July (12th), 590 persons. After this period the number of deaths began to diminish during the latter half of July, the whole of August and September, until in the week ending on the 4th of October 1800, they stood at 92, which is regarded as the normal weekly mortality.

The total number of deaths between the 5th day of April and the 4th of October 1800 amounted to 7813.\*

The siege or blockade lasted from the 6th of April to the 5th day of June,—sixty days,—so that the ravages of the fever among the inhabitants of Genoa continued long after the evils and miseries generated by the siege had ceased to operate.

It is calculated by Rasori, that at least 20,000 persons had been attacked by this fever. If so, the mortality, 7813, must be regarded as high, being

\* In the paper from which these numbers are taken, the total amount is stated to be 8414. But this Rasori thinks is owing to an error in summation.



equivalent to 39 per cent., or more than one in three. From various considerations, and making allowance for ordinary mortality by other diseases, Rasori reduces the mortality by the fever to 20 per cent., or one in five.\*

When this fever made its first appearance in August 1799, with pain of the head more or less severe, ringing in the ears, great muscular weakness, small, feeble, quick pulse, sometimes articular pains, foul tongue, restlessness, and sleeplessness, Rasori, and probably other physicians, looked upon it as an asthenic disease, or one connected with, and originating in, great debility; and he accordingly employed for its treatment various corroborant, tonic, and stimulating agents, commonly decoction of bark with anodyne liquor or laudanum, or both, with wine and appropriate diet, that is animal soups, prohibiting strictly all watery drinks. Under this method of management, the symptoms became unequivocally worse. This deterioration in the condition of his patients appeared so manifestly to depend on the treatment, that he, after some consideration, changed it entirely, and substituted copious acidulated drinks, the use of the neutral salts, tamarinds, nitre, and strict low diet. Hitherto he had not employed bloodletting. The symptoms became moderated, and in ten, twelve, or fifteen days, the improvement was manifest, and the evacuations were abundant towards the close of the disease.†

\* *Storia della Febbre Petecchiale di Genova, anni 1799 et 1800, &c.* Di G. Rasori. In *Raccolta*, Tomo Nono, p. 301.

† *Ibid.*

Cases of this Fever continued to prevail during the autumn months, Rasori thought in greater numbers, and several he saw in the course of the winter 1799–1800. Rendered more circumspect by the experience derived from the previous cases, he prescribed a debilitating regimen, and thus clearly perceived the nature of the Fever, which tended to run a definite course. The cure was not difficult, and convalescence followed. The idea, nevertheless, began to rise in his mind, that there was at work a general cause inducing an epidemic distemper.

It was only, however, at the end of winter and the beginning of spring, April 1800, when the numbers of Fever cases became greater in the city of Genoa; and when a Fever of the same character had been causing great ravages in Nice, when the head symptoms appeared to be more severe, and the second stage of the disease was attended with more alarming symptoms, in short, the number of deaths increased, that, without changing altogether the kind of treatment, Rasori thought on the means of rendering it more energetic.

With this view, Rasori began to think on the necessity of detraction of blood, and he directed, accordingly, blood to be drawn by cupping-glasses from the shoulders, and leeches to be applied to the temples and the neck, so as to remove eight or nine ounces of blood. Soon, however, it was impossible to obtain leeches, in consequence of the severity of the operations of the enemy. Then he thought on venesection, though he was not unaware of the prejudices against detraction of blood in Fevers, and

especially in those in which petechiæ had either already appeared, or might be expected to appear.

In this apprehension, Rasori shews himself to have been entirely ignorant of the fact that Dover had, in 1709, not only employed, with great advantage, bloodletting to a large extent in the Remittent Tropical Fever of Guayaquil in South America, but that he had recourse, with similar good effects, to the same evacuation in the Spotted or Petechial Fever of this country.\* But such is the fate of Medicine. The experience of shrewd observers and sensible practitioners is lost in succeeding ages from ignorance of their services.

It was the opinion of Rasori that the cause of Fever acted as a stimulus, and that, consequently, the most rational method of cure was to employ means which might counteract the effects of this stimulus, and this power he thought bloodletting possessed. It was necessary, in short, to debilitate or lower the system ; but in doing so, to do it cautiously, that is, to employ only moderate bloodlettings.

Next to bloodletting, however, he had recourse to antimony, especially tartar emetic and kermes mineral (Precipitated Sulphuret of Antimony). The former, particularly, he found most suitable, and he prescribed it from the first in doses of four, six, eight, and sometimes more grains. In other instances, where it was not practicable to give the medicine by the mouth, he directed it to be administered in clyster.

\* The Ancient Physician's Legacy to his Country. By Thomas Dover. The Fourth Edition. London, 1733. P. 78. See the case of John Dineley Goodere, pp. 121-127.

It is needless, however, to dwell on this part of the subject. It is enough to say that Rasori employed freely not only tartar emetic but kermes mineral, nitre, and purgatives even of the most drastic kind. The kermes was generally given combined with nitre, one grain or one grain and a half of the antimonial salt being associated with one scruple of the latter.

His experience of these remedial agents led Rasori to distrust entirely the remedies supposed to possess tonic and stimulant properties, as bark, opium, camphor, wine, and spirits, and, in the treatment of such diseases, to place all his confidence in those methods and agents which exercise a lowering, debilitating, and evacuant effect. It was in truth a depriment, reducing or lowering method of treatment. Rasori, however, still looking to its power in opposing and resisting the stimulus of the morbid action, called it **CONTRO-STIMULANT**, and the agents employed, **CONTRO-STIMULI**. Of little moment is it what is the name to be applied. The method was, to all intents and purposes, what had been called by other physicians **ANTIPHLOGISTIC**; and this Rasori\*

\* "Ma, tutto convenevole che sia il metodo debilitante, o antiflogistico, come suol chiamarsi," &c. Storia, &c., p. 274.

"Il metodo antiflogistico sottraendo opportunamente dal sistema una data quantita di stimolo, rende meno nocevole, vale udire meno eccessiva, l'operazione stimulante della materia vajuolosa," &c. Ibid., 274.

It would not be difficult to prove that the premises assumed by Rasori of the presence of a *Stimulus* in the human body during Febrile and Inflammatory Disorders are not founded on any solid grounds, and are at best only a mode of expression

himself admits, and long subsequently Tommasini allowed.\*

The practice of Rasori in Genoa, made known as it was immediately after all over Italy, was a sort of death blow to the system of John Brown. In no long time the same physician published an account of experiments made on the treatment of peripneumony and pleurisy by means of bloodletting and the use of Foxglove. By these proceedings, Rasori may be said to have entirely abandoned the system of Brown. This disposition was still more decidedly evinced by his observations and experiments on the effects of various purgative and saline remedies, of lowering effects, in the treatment of other inflammatory diseases;† of gamboge in the cure of intestinal discharges; and, above all, of purgatives and neutral salts, in that of dropsies called sthenic, or supposed to belong to the order of Inflammatory dropsies. It

employed to render palpable what is altogether unknown. But this is quite unnecessary. It is of no consequence to the historical correctness of the hypothesis whether there be a Stimulus or not. It is sufficient that the author imagined or believed that a Stimulus was present and operating during the existence of the disease. It would indeed resolve itself into a verbal discussion on the meaning of the term *Stimulus*, which appears often to be the same as Morbid Poison.

\* Della Nuova Dottrina Medica Italiana. Prolusione alle Lezioni di Clinica Medica nella P. Università di Bologna. Per l'Anno Scolastico, 1816, 1817. Del Professore Giacomo Tommasini. Bologna, 1817. Nota 13, p. 65.

† Della Azione della Digitale nel sistema vivente; Dell uso della Gomma Gotta nei Flussi intestinali, &c.; e della cura delle Peripneumonie Inflammatorie col Tartaro stibiato. See Tommasini Prolusione, Nota 8va, p. 53 and 54.

would lead me, however, into a field too extensive, were I to trace the subsequent course of this schism from the school of Brown.\*

It is sufficient to say, that, in 1800, Guani, who was a practitioner in Sestri,† a small town in the neighbourhood of Genoa, had occasion to treat the same Febrile disorder, and to suffer an attack of it in his own person. The disease, indeed, seems to have prevailed over the whole coast district of the Gulf of Genoa, from Nice on the west side to beyond Genoa on the east, and was justly enough named by Guani the Ligurian epidemic. Guani, who published his *Account of the Ligurian Epidemic* in 1801, admits the general accuracy of the description given by Rasori, but thinks that its character was not so decidedly sthenic or inflammatory as by Rasori it was represented to be.‡ In several instances, he found copious bloodlettings to be hurtful, and he

\* See Note R.

† Guani represents himself, at the time at which he wrote on the Ligurian Epidemic, to be living obscurely in a corner of the Duchy of Genoa. “Uno che vive oscuramente nell’ Angolo de Ducato di Genova.” *Introduzione a lo Discorso Del Stimolo et Contro-stimolo.*

LIGURIA was the name applied by the Romans to the district inhabited by the LIGURES, probably a Celtic nation. The district was divided into two parts; one, sea coast, bounded on the west and north by the Alps; on the east by the river Magra and part of the Apennines; on the south by the sea, the Gulf of Genoa; the other inland, extending as far as the Po. At present it is mostly a sea-coast district.

‡ *Saggio e Riflessioni sull’ Epidemia della Liguria. Ossia, Saggio di una nuova teoria sulle malattie epidemiche e contagiose. Par G. B. Guani. Genova, 1801.*

treated his patients in general by means of emetics and diaphoretic remedies. The evidence adduced by Guani, nevertheless, was not less hurtful to the stability of the Brunonian doctrines than had been that given by Rasori, and his experience led him to call in question the hypothetical distinctions of *Stimulus* and *Contra-stimulus*, by which Rasori endeavoured to make his reasonings intelligible. At a subsequent period, indeed, Guani pronounced the distinction into *Stimuli* and *Contra-stimuli* to be inadequate and insufficient to embrace all the forms of disease and remedial agents; he proposed the new Diathesis of Irritative Pathological Condition as necessary to account for the facts; and he expressed the opinion that the distinction of Remedies into Stimulant and Contra-stimulant\* is contradicted by Logic, Physiological, Pathological, and Therapeutic. Guani allows, nevertheless, that the idea of Irritative Diseases is not new, and he admits that it had occurred to Gaubius; he might have added many more.

After Guani came Giacomo Tommasini, who, in his Treatise on Yellow Fever, published in 1805, maintained the inflammatory characters and effects of that disorder, and the consequent necessity of contra-stimulant or antiphlogistic measures. In the end of 1806, the same physician maintained in the Academy of Parma a Thesis or Discussion upon the Theory of Contro-stimulus and its application to Practical Medicine. It appears that at this time, Tommasini entertained some doubts whether his ideas

\* Del Contro-stimolo et delle Malattie Irritative. Opuscolo Del Dottore G. B. Guani. Raccolta, Tomo Quinto, p. 86-96.

on the nature of the Contra-stimulant practice were quite the same as those of Rasori, and whether the doctrines of the latter were perfectly well understood. The question, though relating to certain differences in Pathology and Therapeutics, was altogether nugatory and of little moment. The truth, in short, is, that many of the Italian physicians, both Teachers and Practitioners, after adopting and following for some years the speculations taught and the practice recommended by John Brown, had become in some degree satisfied of their insufficiency and their practical errors, and had come back to the doctrines and practice of other physicians, and especially those of the British school. But this return to common sense and sound practice was disguised under the name of the New Italian Medical Doctrine.

Notwithstanding this manifest objection to the newness and originality of the Italian Medical Doctrine, Tommasini became, in the course of a few years, the great expounder and illustrator of this doctrine. He became Professor of Physiology in the University of Bologna, and eventually of Clinical Medicine. In his lectures on the former subject, he treats in considerable detail of the theory of Stimulus and Contra-stimulus, the doctrine of Excitability, and the use of Contra-stimulants or Depriments, all under the idea that he is unfolding a series of doctrines altogether new and never previously heard of in medicine.\* In a professed Introductory Discourse

\* *Lezioni Critiche di Fisiologia et Patologia del Professore Giacomo Tommasini, Membro del Collegio Medico di Parma ed Attuale Rinomatissimo Professore di Clinica Medica Nella*  
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to the Lectures on Clinical Medicine for the academical year 1816–1817, he gives a detailed exposition of the New Italian Doctrine, shews how it differs from the doctrine taught by Brown, and fails not to claim for the former both novelty, originality, and superior excellence in pathological principles and practical applications.\* In 1820, he published at Pisa the First Part of a Treatise or Essay on Inflammation and Continued Fever. His object in this first part of the Essay, confined to Inflammation, is chiefly to prove that inflammation always consists in augmented action ; to show the danger of allowing inflammatory diseases to proceed unchecked, or of treating them by means of stimulants and tonics ; to remind the practitioner in how many morbid states of the human body it performs an im-

Pontificia Università di Bologna. Tre Volume. Nella Raccolta di Opere Nuove Mediche Moderne Italiane, Tomo I. et Secondo. Bologna, 1827.

\* Della Dottrina Medica Italiana, Prolusione Alle Lezioni di Clinica Medica Nella P. Università de Bologna per l'Anno Scolastico, 1816–1817. Del Professore Giacomo Tommasini. Bologna, 1817. Pp. 128.

Besides the works now referred to, Tommasini has published a great number of Essays and Treatises. The work on Inflammation and Continued Fever, immediately to be mentioned, was completed and published in 1826 and 1827 in two volumes 8vo. His Dissertations and other writings relating to the peculiarities of the New Italian doctrine occupy five volumes 8vo. The whole question, nevertheless, may be comprised in one line ; namely, that whenever the physician is satisfied that there is inflammation, then he is to employ the Contra-stimulant, that is, the antiphlogistic method of treatment, dietetic and medicinal. Tommasini also translated into the Italian language the lectures of Dr Thomson on Inflammation.

portant part; to explain, that there are two general forms of the process, the common or **STHENIC**, or that with vigorous action, and the **MALIGNANT**, **PUTRID**, **NERVOUS**, or **ASTHENIC**, or that with low and imperfect action; that the Malignant or Nervous does not depend, as represented by Brown, on defect of excitement, but rather, as the previous physicians taught, on the presence of a putrid, malignant, deleterious principle in the blood. But lest this admission may seem to denote the disposition to assign to the humoral pathology a greater degree of importance than it merits, Tommasini adds to this idea another condition, which he thinks is necessary in order to form a just conception of the nature of the inflammations accounted Malignant. Tommasini then states the opinion that the inflammations called malignant or gangrenous, ought to be considered under two different aspects, which hitherto, from neglect of a rigorous analysis of facts, have been imperfectly distinguished. Of these two aspects, the first relates to the basis, constitution, or temperament of the solids and fluids of the individual in whom an inflammatory attack arises; the second relates to the parts which are idiopathically deeply attacked by inflammation.\*

As to the first relation, Tommasini thinks that the organic basis or tissue, in which an attack of inflammation arises, necessarily presents the union of solids and fluids,—of the blood as well as of the primitive fibre and of vessels; and through various considerations he arrives at the conclusion, that these

\* Dell Infiammazione e della Febbre Continua. Pisa, 1820. Capitolo vii., § 50, pp. 119, 120.

conditions of the organic basis, including the blood, form a state analogous to, or the same as that in Scurvy.

With regard to the second kind of relations of the Inflammations accounted Malignant, namely, the quality of the parts deeply attacked by the phlogistic process, he is of opinion that the circumstance of the Nervous System being either seriously disordered, or deeply attacked by Inflammation, often gives rise to many of those phenomena by which Malignant Inflammation is accompanied. This condition of the Nervous System, Tommasini further distinguishes into two forms; the first, when some nervous filaments of great importance, and of extensive relations, are merely stretched, compressed, vellicated, or irritated by the phlogistic process; and the second, when the nerves themselves are idiopathically affected by inflammation. In the first case, amidst violent convulsions, *subsultus* more or less considerable, or with great depression of the pulse and strength, and under the most nervous and malignant symptoms, the inflammation advances to its ordinary terminations, namely, suppuration, adhesion, or induration of parts; the process continues phlogistic to the last, and life is terminated without the part proceeding to gangrene, either by violent convulsions or by paralysis of the nerves most intimately connected with life. In the second case, on the other hand, when the nerves themselves are idiopathically attacked by the phlogistic process, not only phenomena indicating the greatest vital depression are urgently developed, but the parts which

derive life from the inflamed nerves pass rapidly into gangrene.\* In this second class of cases, in short, the inflammation in the medullary part being essential and idiopathic, the inflammation has a progress not only rapid but peculiar, and for the reasons already suggested, and from the circumstance that it is almost incapable of the ordinary terminations, susceptible only of the most rapid and the most fatal of all ; hence incurable by its own nature.

The author examines this subject with great learning and acuteness. But neither can the facts stated be said to be new, nor the doctrines to be original. Tommasini visited England in 1820 and 1821, and had ample opportunities of seeing the practice of the English and Scottish physicians, especially in the public Institutions. In one of two of his subsequent publications he labours with great zeal and diligence to prove two rather inconsistent things ; namely, the great originality and superiority of the Italian doctrine, and its resemblance to the pathological principles and practical observances of the British physicians ; and in another he gives a view of the points of agreement and discrepancy† between the English and Italian methods of cure. He was an active,

\* *Della Infiammazione e della Febbre Continua, &c., Capitolo vii., § 51, pp. 123, 124.*

† *Del Metodo di Curare, Dell' Insegnamento Medico-Clinico e di Alcuni Stabilimenti Di Pubblica Beneficenza Osservati in Inghilterra. Discorso pronunciato Nella Clinica Medica della Università di Bologna. Il xxvi. Marzo, 1821. Dal Professore Giacomo Tommasini. Bologna, 1821, 8vo, pp. 82.*

*De Congruentia et Discrepantia Inter Anglicam et Italicam Medendi Rationem. Epistola viro celeberrimo Astley Cooper. 23tjo Januarii 1821.*

enthusiastic person ; but his zeal occasionally was greater than his judgment ; and he has not convinced or converted all the Italian physicians to his modes of thinking.\*

Bufalini had never shown any favour either to the doctrines of Brown, or their modified offspring, the new Italian medical doctrine ; and he took various occasions, as has been already in some degree seen, to show the erroneous nature of the former, and, by studying to give correct views on pathology, indirectly to depreciate the latter. But the persons among the physicians of Italy who shewed most strongly their dislike of the new fancies and their appreciation of the old methods, were Locatelli and Speranza, the former Professor of Clinical Medicine at the Civil Hospital at Milan, the latter Professor of Special Therapeutics and Clinical Medicine in the Ducal Academy of Parma. As the acknowledgment of the latter confirms what has been already said of the character of John Peter Frank, as well as his account of Locatelli, it is right that the words of Speranza should be given.

“ A pupil of John Peter Frank, at a time when the seeds of the Brunonian reform were spreading in Italy, I was taught, when not yet a physician, to understand that the school of systems in Medicine is at all times the school of errors. Often that incomparable teacher repeated to me, that such systems spring only from an insane passion for making a name, aided by poverty in true knowledge of facts. He added that there were systems created by spirits too fervid and imaginative, and which were easily

\* See Note S.

embraced by moderate minds ; that every age had adopted new doctrines, which were in the next age rejected and thrown into oblivion ; that we laugh at the theories of our predecessors, yet ourselves give posterity equal occasion to laugh at ourselves. Medicine, he continued, is nothing but a simple and sincere history of the maladies of the human race, of the causes which have produced them, when we can discover and understand these causes, of the phenomena by which they are attended, and of the remedies by which they are overcome, either with or without the assistance of nature. My principles in this part of medicine, he said, with the peculiar sincerity of his heart, are all the immediate result of constant and repeated observations, the greatest part of which, however, have been made by the most celebrated and judicious practitioners from Hippocrates to our days."

"With instructions so valuable, I went to the Clinical Course of Professor Locatelli, in whom, as before completing his studies he had spent a few years in England and Scotland, I expected to find a partizan of the Brunonian doctrine, which was daily acquiring new proselytes.\* But what was my surprise to find this excellent Clinical teacher, following the medical practice of Tissot and Borsieri,

\* F. Giacomo Locatelli, who was Clinical Physician or Professor at the Civil Hospital of Milan in 1816 and onward, states that he witnessed, in 1786, the first experiments made in the Clinical Ward of the Royal Infirmary of Edinburgh, to verify the observations of Withering upon the use and effects of Foxglove. *Omodei Annali Universali di Medicina*, Vol. XVIII., p. 24.

whose pupil he had been. Contrasting the practice of Locatelli with that of other physicians, I perceived by clear signs that its results were most satisfactory in its favour, whereas the contrary was the case on the part of the adherents of Brown. Upon asking the cause of so great diversity, this respectable person answered, that according to theories, ‘The sick are cured in books and die in beds ;’\* a sentence which I have never forgotten in the whole course of my life.”†

Speranza afterwards states that the valuable instructions of Locatelli, the deliberate anti-Brunonian reflections of Sacchi, of Villa, of the elder Strambio, and especially the very great superiority in practice of physicians who trusted to observation and experience, compared with that of the followers of the Brunonian reform, awakened in him, when a practitioner and an hospital physician, repugnance rather than attachment to any new system. Physicians of the former class he saw cure by the antiphlogistic method attacks of pneumonia, nervous fevers, and dysenteries, while those of the latter class, treating by stimulating, heating remedies, the same diseases, increased the number of deaths.‡

It would be in this sketch an omission not to remark, that while the Brunonian speculations and

\* *Aegri curantur in libris et moriuntur in lectis.*

† *Al Chiarissimo Professore Giacomo Tommasini Lettera del Professore Speranza Intorno a Cio che lo riguarda sull’ Opinione in Medecina. Raccolta, tomo quinto, p. 260.261. (Giornale Critico di Medecina Analitica).*

‡ *Ibid. P. 261.*

practice were eschewed, rejected, and despised by some of the most judicious and learned physicians of Italy, they were received and made the subject of familiar discussion by all the unprofessional persons in Italian circles. In the heat of the Brunonian reform there was not a divine, a lawyer, a citizen, an artizan, or even a young lady, who had not on their lips the new terms, and could discourse fluently and confidently of the qualities of musk, camphor, opium, and all sorts of agents supposed to possess diffusible stimulating properties.\* The ladies especially took great interest, according to John Peter Frank, in the new doctrines.

We have here a pointed illustration of the principle, that the zealous support of a doctrine or set of dogmas is very much in the inverse ratio of the information of the supporters upon the merits of the question, and their fitness for judging of these merits. Clergymen and lawyers are generally well-educated and intelligent persons, and capable of forming sound and just opinions on subjects not remote from their own studies and habits. But in this instance the subject was such, that it cannot be imagined, by any stretch of indulgence, that the parties who were most prompt and decided in forming and pronouncing judgments, had been by previous habits and information rendered capable of judging, in any manner, of the merits of the question, which they presumed to defend. On the qualifications of the citizens, the artizans, and the ladies, for forming a just estimate

\* Ignazio del Monte in the letter of Carlo Speranza to Tommasini, already quoted. *Raccolta*, Tomo Quinto. P. 274.



of the merits of the Erythronian doctrine it is unnecessary to enter any opinion.

To pursue this subject further, however, is unnecessary. It has been seen that the doctrine of Brown fell in Italy by the hands of its early supporters. The new Italian Medical Doctrine, so far as it exists as a separate and original system, is destined not to enjoy a prolonged existence. Already it may be said to be gradually passing into oblivion. The Hospital physicians and practitioners in the Italian States are gradually returning to the therapeutic methods of France and Great Britain.

We have at length traversed the rugged and thorny region of controversy and misrepresentation ; of rival, conflicting, and disputed claims ; of alleged discoveries, reforms, and improvements in Medical Theory and Practice. The discussions into which these circumstances have led, have carried us considerably beyond the time at which we left William Cullen, calmly discharging his duties as a public teacher, and the principal medical authority in the capital of Scotland.

Meanwhile the course of events, as well as of time, was moving onward with Dr Cullen and his family. His eldest son, Robert Cullen, who was bred to the Scottish Bar, had, on the fifteenth day of December, in the year 1764\* become a member of

\* Allusion to this event is made in a letter from Dr William Hunter to Dr Cullen in Volume First, page 553, Appendix, where he thanks the young advocate for his Law Thesis.

the Faculty of Advocates,—a step equivalent to that of being called to the Bar in England,—and was gradually but steadily making known in the exercise of his profession those talents and that knowledge of law, by means of which he subsequently rose, in 1796, to be one of the Supreme Judges of Scotland. Other two sons, namely, Henry and Archibald Cullen, were preparing themselves for following the profession of Medicine.

Henry Cullen appears to have possessed a considerable amount of the paternal genius, and had devoted much attention to the study of Natural History and Botany; and early in the year 1778, when Dr Ramsay, then Professor of Natural History, was so unwell and so frail, that it was believed he could not recover, Dr Cullen had thought of his son making application for the chair when vacant. Other candidates were also mentioned; and among these were the Rev. John Walker, minister of the parish of Moffat, a man who had already rendered himself known by his devotion to the subjects of Botany, Mineralogy, and Zoology, and Dr Daniel Rutherford, then a young physician well known for his knowledge of Chemistry and Botany. Mr Walker was a friend of Dr Cullen, and appears to have been rather disconcerted, when he learned from the Doctor the views which he had entertained regarding Henry Cullen, and to have been unwilling to appear as a competitor against the son of his friend. Various friends, however, of Mr Walker, who knew his qualifications, and were aware that he had been for years preparing himself for teaching Natural History,

offered to interest themselves on his behalf; and in consequence of this encouragement he had resolved to offer himself as a candidate, even though the son of Dr Cullen were to be a competitor. In this state of matters, however, Mr Walker was relieved from all difficulty, by learning on the 7th of February 1778 that Henry Cullen had declined appearing as a candidate. The Rev. Mr Walker was eventually appointed to the office in the commencement of the year 1779.\*

At this time, namely, in February 1778, Henry Cullen was young, and had not yet completed his studies; and it was only two years subsequent to this date, that is, on the 24th day of June 1780, that, after completing the usual course of study, undergoing the prescribed examinations and defending his Inaugural Dissertation *De Consuetudine*, he received the degree of Doctor of Medicine from the University of Edinburgh. Of this Dissertation his father was wont to speak in terms of commendation; and to its pages as containing a good exposition of the Effects of Custom and Habit in general upon the Mental and Corporeal Functions, and laying a foundation for the doctrine of explaining peculiarities in Temperament and Idiosyncrasy, Dr Cullen subsequently, in 1789, refers for full information on this subject;† expressing at the same time the hope that his son would soon give another edition of the Dis-

\* See Note T.

† A Treatise on the Materia Medica. By William Cullen, M.D., &c. In two volumes, 4to. Vol. I., p. 123, Chapter I., Section I.

sertation in a more complete form in the English language.

This might appear to be the sentiment of a parent pleased by the first efforts of his son in a difficult and somewhat obscure department of Mental and Nervous Physiology. It has, however, been stated in a former part of this work, that most of the Materials of the Inaugural Dissertation of Henry Cullen were derived from fragments of an Essay by Dr William Cullen, on the *Effects of Custom upon our Corporeal and Mental Functions*, which appear to have been written at an early period of the life of the author.\* It is impossible to doubt that, in the first preparation of the Dissertation *de Consuetudine*, Henry Cullen, the son, would enjoy the benefit of the matured thoughts, reflections, and experience of the father; and in this point of view, it was at once natural and proper for the latter to refer to the Dissertation of his son, as containing on the Influence of Custom upon the Corporeal and Mental Functions of Man, views which Dr Cullen himself had formed by observation and reasoning, and views which were at that time the most recent, the most complete, and the most just. The Dissertation gives evidence of observation and reflection, contains several ingenious remarks and some good general deductions; yet cannot be said to be eminently distinguished for original views. Probably it suffers by being compared with writings which have appeared since the date of its publication. It is scarcely necessary to say that the expectations of the father as to

\* Life of William Cullen. Vol. i., p. 319.

the republication of this performance were not realized.

To conclude what relates to Dr Henry Cullen, he became on the 5th of November 1782 Fellow of the College of Physicians of Edinburgh, and was subsequently appointed Physician to the Royal Infirmary. In November 1786, when the Chair of Botany became vacant by the death of Dr John Hope on the 11th of that month, Dr Cullen made an attempt to obtain the appointment for his son Henry, by applying to Henry Dundas, who was at that time the supreme disposer of all preferments and offices of emolument in Scotland. But this gentleman was of opinion that Dr Daniel Rutherford, who had been candidate for the chair of Natural History in 1778, had at that time, 1786, superior claims to the office; and Dr Rutherford accordingly was appointed Professor of Botany in November 1786.\*

Dr Henry Cullen had no opportunity again during the few residual years of life, of offering himself for any public situation, or discharging the duties of any public office. It appears that he had intended after the death of his father to deliver lectures on the Practice of Medicine. But all his prospects were frustrated by early death. He departed this life on the 11th day of October 1790, a few months after the death of Dr Cullen himself. He was the only one of the seven sons of Dr Cullen who adhered to the profession of medicine.

Archibald Cullen, the second of the sons of Dr Cullen who studied Medicine, received from the

\* See Note U.

University of Edinburgh the degree of Doctor three months after his brother Henry, on the 12th day of September 1780, after defending his Inaugural Dissertation *de Frigore ejusque Effectibus in corpus humanum*. This Dissertation, which is dedicated to William Hunter, the early pupil and attached friend of his father, is a respectable performance, and gives a pleasing idea of the talents of the author.

Whether it be that the subject is one more intelligible and more capable of illustration, or that it is treated with greater ability, the Dissertation appears to me to be more interesting than that of his brother. The author thinks that cold causes death, not by freezing and coagulating the animal fluids as some have believed, but by its sedative effects acting on the nervous power and the vital principle. He makes, indeed, a sad mistake in supposing that wine and spirituous liquors, while they excite the circulation of the blood, render the system less liable to be acted (*opportunum*) on by cold, and that men warmed with much wine can withstand a certain degree of cold which would otherwise have destroyed them.\* Multiplied experience has shewn that the fact is, as nearly as may be, the reverse ; and that persons who, under exposure to great degrees of cold, have recourse to wine and spirituous liquors, invariably became sooner victims to the destructive effects of cold than those who avoid the use of these articles. Notwithstanding this mistake, however, the Dissertation possesses considerable merit.

Archibald Cullen, however, soon gave up the

\* *Dissertatio Inauguralis*, p. 43.

medical profession and studied law in London, where he eventually rose to be a Chancery barrister of eminence, and published a Treatise on the Bankrupt Law of England.

Both of these young men, and especially the latter, appear to have been great favourites of William Hunter.\* Both had spent some time with him in London, and both had received from him various marks of kindness.†

Philosophers and poets, moralists and theologians, speculative observers and men of the world, have all united in directing the attention of their fellow creatures to the inevitable evils, the irreparable losses, that occur to those who have enjoyed or endured a prolonged term of existence. These various authorities have reminded us, that as man becomes aged, he beholds, successively withdrawn from the present scene, esteemed friends and associates with whom he had commenced the journey of life, various relatives attached by ties of affinity and kindred; friends whose counsel he was in the habit of seeking, who rejoiced in his prosperity, and sympathized in his trials and sufferings.

\* To prevent mistakes, it may be well to mention, that on the 25th of June 1781, exactly one year after the graduation of Henry Cullen at Edinburgh, Edmund Cullen, an Irish gentleman, A.B. of Trinity College, Dublin, received his degree from the University of Edinburgh, after defending his Inaugural Dissertation *de Aere et Imperio ejus in Corpora Humana*. This gentleman was on the 21st March 1786 appointed Professor of Materia Medica and Pharmacy in the Medical Faculty of the University of Dublin.

† See Letters in page 562 and 563 of Volume First.

Ut vigeant sensus animi, ducenda tamen sunt  
 Funera natorum, rogos adspiciendus amatæ  
 Conjugis et fratris, plenæque sororibus urnæ.  
 Hæc data pœna diu viventibus, ut, renovata  
 Semper clade domus, multis in luctibus inque  
 Perpetuo mœrore et nigra veste senescant.

*Juvenalis, Sat. x., 240.*

Of this delineation some of the incidents were now approaching Dr Cullen. Of one esteemed and distinguished friend, David Hume, the philosopher and historian, he had witnessed the last days and alleviated the final sufferings in August 1776; and an interesting sketch of this final scene he sent to one with whom he had long been connected in the twofold relation of instructor and friend.\* This very friend, however, was himself at this time suffering from the inroads of that disease which so often assails those, in whom the mental faculties have been too continuously and intensely exerted; and in the course of six years his health had become so much impaired, and his tenure of life so precarious, that it became difficult to say how soon he might be called away from that profession, of which he had been long a most active and useful member.

William Hunter, who had been a pupil of Cullen when acting as a medical practitioner in the town of Hamilton in 1737, had gone to London in the summer of 1741, with the intention of completing his medical and surgical studies, by attending the hospitals and lectures of the metropolis. A letter of

\* See Volume First, Note N., p. 607. Letter regarding the last days of David Hume.



introduction from Foulis the printer at Glasgow was the means of introducing Mr Hunter to Dr James Douglas, known at that time as a skilful and zealous anatomist, an experienced accoucheur, and the friend of Cheselden ; and the effect of this letter has been shortly recorded in the former part of this work.\* This is not the place to trace the history of William Hunter, further than as it is connected with that of his illustrious instructor and friend.† It is sufficient to say, that, by unremitting diligence and perseverance, by a rare combination of bold enterprise and judicious management, William Hunter, without patronage, and unaided by public means, succeeded in creating in the metropolis, upon a stable foundation and an enlarged scale, a school of anatomy, didactic and practical, unequalled in this country for its efficiency, its extensive utility, its scientific character, and its permanent influence on the medical profession in London. With superior talents for the organization of a system of scientific and professional instruction, he taught his brother, John Hunter, to be most eminent in anatomy, physiology, and pathology ; he successively brought forward William Hewson, William Cruikshank, and John Sheldon, whose anatomical writings may all be justly said to have originated in the school of Hunter ; and his nephew

\* Life of Cullen, Volume First, p. 13. See Note W.

† For detailed information on William Hunter, see *An Account of the Life and Writings of the late William Hunter, M.D., &c.* By Samuel Foart Simmons, M.D., &c. London, 1783, 8vo ;—and *Edin. Medical and Surgical Journal*, Volume Sixty-ninth, p. 413.

Matthew Baillie had, mostly under his inspection, acquired that minute and accurate knowledge of Morbid Anatomy which rendered him the highest authority on that subject at the close of the eighteenth century. It is gratifying to have to record that so much disinterested labour and exertion was not altogether unrewarded. His professional success, though not greater than his meritorious services, was very considerable ; and though he entered London, in 1741, an unknown and unfriended young man, he rose in the course of less than twenty years to be the first and most trusted physician-accoucheur in the metropolis of England. In August 1762, Dr Hunter was the professional attendant at the confinement of the Queen.

The labours of a life of incessant activity in teaching anatomy and discharging the practical duties of the profession, nevertheless, began, after the lapse of thirty years, to shew their effects upon the health of William Hunter. About the year 1773, when in the fifty-fifth year of his age, Dr Hunter began, notwithstanding an habitually abstemious mode of living, to suffer much from symptoms of wandering gout. In the course of six or seven years, the fits of this malady became more frequent, affecting sometimes the limbs and sometimes the stomach. Yet, though all this was manifestly the result of the brain being overworked, and great and uninterrupted mental exertion in the twofold duty of teaching and practice, Dr Hunter gave himself no respite, little relaxation. It is, indeed, not easy for a physician in a large city to do so, unless he were to withdraw

himself entirely from the public. Upon one of those occasions on which indisposition and positive pain obliged him to keep in bed, a lady, who was desirous to obtain the benefit of his advice, insisted on being admitted to the physician, who was himself suffering, thus shewing her confidence in his professional skill. But the human frame, however it may be sustained by strong will and mental energy, possesses only limited powers of resisting the inroads of disease and infirmity ; and in the struggle it at length gives way. The frame of William Hunter, though healthy, had never been robust ; and its powers had been tried by professional labour to their greatest capability. On Saturday the 15th of March 1783, William Hunter suffered so much from headach and squeamishness, that he was compelled to remain in bed. On the following Thursday, the 20th March, he had so far recovered, that he resolved to deliver the Introductory Lecture to the Operations of Surgery. This duty he performed, but he fainted at the close of the exertion. He was conveyed to bed, and, after lingering in great weakness for nine days, he departed this life on Sunday, the thirtieth day of March 1783, aged not quite sixty-five years.

Thus died at an age comparatively early, a man not less remarkable and scarcely less distinguished than his great instructor and friend William Cullen. With natural talents, highly cultivated, and a considerable amount of learning, the characteristic features of the mind of William Hunter were great energy, indefatigable diligence, and perseverance daunted by no difficulties. These were the qualities

by which he effected a great and serviceable revolution in the mode of teaching anatomy in London. The anatomical school which he founded, and which, though first commenced in Covent Garden, was in his lifetime established in Great Windmill Street, long continued to prosper under a succession of teachers little less distinguished than the founder. In that school, John Hunter, William Hewson, William Cruikshank, and John Sheldon, all taught anatomy during the lifetime of Dr Hunter. These were followed by Matthew Baillie, the nephew of Dr Hunter, James Wilson, Sir Benjamin Brodie, Sir Charles Bell, and Herbert Mayo.

The establishment, in 1826, of the London University, since known by the name of London University College, withdrew Sir Charles Bell, by placing him as Professor of Surgery in the newly established institution ; while the erection of King's College, in 1829–1830, by making Mr Mayo Professor of Anatomy in that establishment, carried away the last representative of a long line of able teachers. These events may be regarded as marking the era of the cessation of the Institution originally established by Dr Hunter. The Windmill Street School no longer exists ; but its remembrance will be preserved from oblivion by the names of the men who, for more than fifty years, continued there to instruct numbers of zealous disciples.\*

The beneficial effects of the exertions and services of William Hunter, however, were not confined to the circumstance of providing for the metropolis a

\* See Note X.

succession of well-informed and industrious anatomical teachers. It is not without interest to remark, that the personal labours of William Hunter himself, aided by his assistants and that of his successors, have provided for London, Edinburgh, and Glasgow, Anatomical and Pathological Museums of great extent, and of the utmost service in illustrating the elementary departments of Anatomy, Physiology, and Pathology. The large and extensive collection, made by William Hunter himself, augmented as it was on the death of Mr Hewson, by the collection of that anatomist, was by his will bequeathed to the University of Glasgow, and was transferred to that city in the month of November 1807.\* Mr Wilson, who succeeded Dr Baillie, aware that he was to lose, in teaching, the aid of this valuable collection, laboured with the greatest industry in forming a new museum in the Windmill Street School, most of the preparations being made by his own hands. This collection, which, by most competent judges, was pronounced to be admirably adapted to the purpose intended, was afterwards greatly enlarged by Sir Charles Bell; and in this extensive shape it became the property of the Royal College of Surgeons of Edinburgh, where it now forms one of the most serviceable Museums both in healthy and morbid anatomy in this city. A third collection, still more extensive, more comprehensive than either, is that which was made by John Hunter, which was acquired by the Royal College of

\* Evidence from Minutes of the Senatus Academicus of Glasgow University. Dr Allen Thomson.

Surgeons of England, and which, originally constructed upon an enlarged physiological method, has since been greatly augmented by the labours of the two last conservators,—William Clift and Richard Owen.

Few are the individuals, indeed, of whom it can justly be said, that their labour, their example, and their actual service have conferred so great benefits upon science, and tended in a degree so remarkable to improve and facilitate the means of communicating the correct knowledge of the facts of Anatomy and the principles of Physiology and Pathology, as William Hunter and John Hunter. It may, without exaggeration, be said that both England and Scotland owe to William Hunter, and his brother John Hunter, a debt which has never yet been repaid, and which probably is too great to be repaid. Both were, in the most true sense of that term, benefactors to the human race. Both laboured not only without public encouragement, but in the face of the greatest difficulties and the most chilling and discouraging inattention, in teaching Anatomy and Physiology, and the just principles of Pathology, upon a foundation solid and durable. And both devoted, in the most disinterested manner, the pecuniary means which the exercise of their profession supplied, not in procuring expensive luxuries, not in acquiring large estates and erecting magnificent mansions, but in providing for subsequent generations the means of anatomical instruction upon the same solid foundations on which they had already taught, and the superior advantages of which they had proved by their

own example. The quiet and unostentatious labours of both brothers were such, that they were not calculated to make a striking impression on public observation. It is only after a long course of years that such labours could produce their legitimate fruits. Of the generation before whom they laboured, the admiration was claimed for brilliant victories by sea, while patience was required for abortive and sometimes discreditable and expensive conflicts on land. Money was lavishly wasted; the lives of seamen and soldiers were profusely and unprofitably sacrificed, greatly more by disease than by the shot of the enemy. But amidst this liberal expenditure of life and treasure, it was never thought to be either a duty, or an act of grace, to aid, by the smallest means, two men who were devoting all their energies and much of their time to the task of educating those physicians and surgeons who were to have the care of the lives and health of the soldiers and seamen by whose fortitude and valour all these warlike operations were conducted. Even when William Hunter asked from the Crown the small boon of a piece of ground, on which he offered, at his own expense, to erect and endow a perfect establishment for teaching anatomy and surgery,—one small portion of those crown lands which had been long leased, without revenue, to court favourites, and the leasing of which had been a positive loss to the nation;—the minister, to whom the application was addressed, never deigned to send any answer. Such was the mode in which, during the reign of George the Third, and when the administration was con-

ducted by the Honourable George Grenville,\* the ruling powers rewarded talent, learning, science, and disinterested exertions. Is there any reason to believe that these useful qualities would have been more justly recognised and more highly estimated at the present time?

The death of William Hunter, his friend and pupil, was not the only calamity which Cullen was at this time called upon to endure. In the summer of 1786, he suffered a loss more immediate in the death of Mrs Cullen, who expired at Ormiston Hill on the seventh day of August of that year. This lady is described by all who knew her as a person of great kindness of manner, and who was much esteemed for the attentions which she shewed to those whom Dr Cullen honoured with his friendship. The loss of a mother, though at an advanced period of life, must have been severely felt in a family where there were four daughters, and where the surviving parent was already beyond three score and ten years. It appears, from unquestionable testimony, that, previous to this bereavement, the spirits of Dr Cullen had become depressed, and his bodily health had shewn indications of failure. After the occurrence of this bereavement, this depression of mind and languor of body appear to have continued during the few subsequent years of his life.† Cullen sought for consolation and support chiefly in the society of his daughters, in various rural occupations at his property at Ormiston Hill, and in a literary labour which was destined to

\* In 1765. See Volume First, page 150.

† Testimony of Mr Paul to Dr William Thomson.



be the last of his industrious and useful career. All the time that was not directly occupied in academical duties, he spent at Ormiston Hill, where he greatly endeared himself to the inhabitants of the parish of Kirknewton by the frankness of his manners, his benevolent habits, and the warm interest which he took in all that related to their comfort and welfare.

We come, accordingly, to that era in the life of William Cullen, in which his merits rest not on the uncertain and fluctuating tide of disputed speculations and hypotheses, but on the stable foundation of judicious induction from observation and experience. Notwithstanding the care with which Cullen laboured to make his physiological principles and pathological reasonings rest on matters of fact and observation; notwithstanding the accurate distinctions which he established between theory and hypothesis; notwithstanding the earnestness, the anxiety, and the frequency with which he declared that all his general doctrines were so many general facts, and that in all his applications of reasoning, he was desirous only to present generalized deductions, which might be useful in the subsequent cultivation of medical science, there were persons who, from various causes, in some instances confusion of ideas, in others indolence, and in others incapacity of comprehension, maintained that his writings and lectures were filled with wild speculations, fanciful conjectures, and unfounded doctrines and statements. It is an old contrivance, which has been often practised, and which will be repeated, so long as it is

found that misrepresentation is readily believed, and so long as there are few competent to form, in matters remote from ordinary inquiry, and relating to complicated scientific subjects, a sound judgment, to ascribe to the labours of genius faults and errors which have no existence but in the fancy of these self-constituted judges, and to pronounce to be fanciful and unfounded, principles and doctrines which differ from those usually inculcated and currently received. It is a device, sanctioned by many examples, for obtaining, upon easy terms, a character for superior intelligence, penetration, and judgment,—for the indolent, the ignorant, and the arrogant,—to pronounce confident opinions on the literary and scientific labours of persons in all respects their superiors, except in assurance, effrontery, and self-esteem. So it fared with the writings of Dr Cullen, upon which the opinions of the superficial and the incompetent were more decided than those of the well-informed. Inferences such as those above mentioned, it might be justly imagined, careful and candid examination of the writings of Cullen could not fail to disprove. But if they required a formal refutation, that refutation will be best found in the next performance which came from the hand of this physician.

It has been mentioned in the First Volume of this work that, so early as the summer of 1748, when Dr Robert Hamilton was Professor of Anatomy and Botany in the University of Glasgow, Dr Cullen, then a medical practitioner in that city, undertook, in conjunction with Mr John Carrick, assistant to the

Professor, to deliver a course of Lectures on *Materia Medica* and Botany ;\* and that at a subsequent period, while he was Professor of Chemistry in the University of Edinburgh, in consequence of the death of Dr Alston in 1760, soon after that teacher had commenced his Lectures for that session, as the course suffered a temporary interruption, the Students of Medicine presented to Dr Cullen a petition, requesting him to undertake the duty of completing the course. In compliance with this request, Dr Cullen commenced a course of Lectures on *Materia Medica* in the beginning of January 1761.†

The effect of these instructions upon the auditors has been already mentioned ; and it was further stated, that the impression of the value, the originality, and the great utility of these instructions to practitioners of medicine was so strong and so durable that, after the interval of ten years, two of these auditors thought it would be a meritorious and serviceable act if they published the series of instructions, such as they were taken down from the lips of the lecturer, in order to diffuse, to a more extensive circle, that information, and the knowledge of those doctrines, the benefit of which they had themselves enjoyed. Upon the nature of this proceeding, it is scarcely necessary here to pronounce any opinion. It was fully ascertained that the parties concerned in it were actuated by no improper feeling, no mercenary motive, and no desire to appropriate even the doubtful merits of a work published under such cir-

\* Life of Cullen, Volume First, page 32. † Ibid., page 141.

cumstances. They seem, in their eagerness to make generally known the doctrines and instructions of Dr Cullen upon the subject of *Materia Medica*, to have overlooked the fact, that it was impossible that a work given to the public in the manner in which this was done, could present any thing like a correct view of the Lectures, such as they were delivered in 1761; or that even if it did, such a publication might by no means be the one, which should do credit to the lecturer, or might rise to that standard which he had fixed, as necessary to give a just representation of the state of *Materia Medica* at the time. Though these Lectures were very highly esteemed in 1761, and though they unquestionably contained many doctrines at that time new, and threw aside many doctrines received upon insufficient foundations, they were, nevertheless, prepared in too short a time to be so complete as their author would wish them; they were not illustrated and confirmed, rectified, and amended, by that matured experience with which he undoubtedly desired that they should be; and they were unavoidably carried through the press with all those faults, original and acquired, from which the most finished productions of human genius are never free, when they receive not the final revision of the author.

The publication of these Lectures, in 1771,\* un-

\* Though there is no doubt that these Lectures were published in the close of the year 1771, some copies are of date 1772, and some of that of 1773. The copy in the library of the College of Physicians of this city is of date 1772. But as the apologetic letter of one of the physicians, who undertook the

authorised as it was, did indeed no harm to the character of the author, though their appearance was calculated, with persons not making allowance for the circumstances under which they were ushered into the world, to create an impression unfavourable to him as a correct and well-informed writer. The striking and conspicuous evil was, that the act of publication implied that the mind of William Cullen, and the science of *Materia Medica* had, for the space of ten years, been stationary and without progress. Amidst many inaccuracies of style and composition, nevertheless, many positive errors, and not a few misstatements and contradictions, there was a general spirit of new doctrine, of original independent thinking, of correct reasoning, of subversion of received opinions and vulgar errors, an amount of new facts, sufficient to indicate the character of the author, and to sustain his reputation as a bold, but sagacious and rational reformer in the practice of medicine.

The simple act, however, of publishing without the knowledge, and without obtaining the previous permission of the author, a body of doctrines and facts in *Materia Medica*, in a great degree new and peculiar to himself, became a sort of compulsory reason for the author placing before the profession, at as early a period as possible, that system of theoretical and practical knowledge which it had been the labour of many years to collect and methodize, and which, when published, would powerfully con- publication, is dated 10th January 1772, it is evident that the book was published previously, that is, in 1771. See Volume First, page 611, Note Q.

tribute to render his other writings more intelligible and more complete. The Physiological Doctrines and Principles taught by William Cullen had now been before the medical students attending the University since 1772, that is for at least fourteen years. The Pathological and Therapeutic Doctrines, both General and Special, had been rendered more or less familiar to the junior members of the profession by the First Lines since 1777 and 1784, when the work was completed; and the author was annually giving to these doctrines new confirmations, more ample expansions, and more just applications, in his Clinical Lectures, delivered from season to season on the cases treated in the Royal Infirmary, till the year 1775, when he ceased to deliver Clinical Lectures. On many subjects, however, which came under consideration in both courses of Lectures, it was manifest that more ample and detailed explanation was requisite, in order to render the therapeutic instructions fully profitable to students and practitioners. It had, in short, become, in the lapse of years, an act of justice, not only to the reputation of William Cullen, but to his own works, to his pupils, and to the profession, that his doctrines on the subject of MATERIA MEDICA should be published in a form so complete and improved, as might present a nearly accurate impression of the mind of the author in regard to that department of Medical Science.

MATERIA MEDICA was, indeed, one of the branches of Practical Medicine, the knowledge of which Dr Cullen had most assiduously cultivated. But this division of medical science he had not cultivated in

an isolated manner. He had studied it both generally and in parts, in connection with the two departments of Physiology and Pathology. Nor had Dr Cullen only studied this department himself. By his example, by his advice, by his instructions, he had urged many of his pupils to devote their attention to particular divisions of the *Materia Medica*, and had been patiently, during a long series of years, laying the foundations of that system of practical therapeutical knowledge which has been gradually and almost imperceptibly embodied with the Practice of Medicine in the nineteenth century. Cullen, in truth, had never, in the twelve or thirteen years, which elapsed after the unauthorised publication of his Lectures on *Materia Medica*, lost sight of the necessity of placing before the profession such a system of facts and doctrines on that subject as might be worthy not only of the early character of his instructions, but of those sound, correct, and matured views which reflection, aided and rectified by much observation and experience, had enabled him to take on the physiological action and therapeutic effects of remedial agents.

It was under the circumstances now mentioned, and with the objects now stated, that Dr Cullen resumed the labour of digesting and correcting his materials. He speedily found, however, that, as he proceeded, the labour assumed the character of a work in a great degree new. The original materials turned out to be of little avail. More than twenty-four years had elapsed since the date of the first delivery of the Lectures in 1761, and fourteen years

had elapsed since the publication of the unauthorised edition of these lectures; and it is easy to perceive that any attempt merely to correct, amend, and enlarge these lectures must have been more difficult than to write the work anew. We find, accordingly, from the testimony of Mr Paul, the gentleman who performed the greatest part of the duty of amanuensis, that in the course of the four or five years which elapsed between 1785 or 1784 and the date of publication, many parts were written entirely anew, and the whole underwent changes so multiplied, so considerable, and so complete, that it gradually assumed the appearance and characters of a new work. Dr William Thomson was informed by this gentleman, that during three or four summers, Dr Cullen devoted all his leisure time, and the winter evenings of the same years, to the work of writing and making additions, corrections, and alterations, and that these corrections, alterations, and additions, were so frequent and so considerable, that many parts of the work were written not less than three times. It was, further, a fact well known among the profession in Edinburgh, and especially the friends of Dr Cullen, that he had been for several years engaged in this task, the completion of which was contemplated with considerable interest by those who wished well to the progress and improvement of scientific medicine and the diffusion of accurate knowledge among medical practitioners.

At length, in the month of March 1789, the work appeared in two volumes quarto, under the title of a *Treatise of the Materia Medica*, by William Cullen,



M.D., &c. The author states in the preface, that, finding it not possible to give, in a satisfactory manner, a correct and complete edition of the Lectures which were published under his name in 1771, with the necessary corrections and supplements, he had long abandoned that idea, and judged it more proper to give a work almost entirely new. In this light it was unquestionably entitled to be regarded.

In this preface, Dr Cullen further states shortly the reasons which induced him to abridge the lists of the *Materia Medica*, and to omit certain articles received by other authorities; and he explains the general method of procedure observed by him, and various other points relating to his mode of treating the subject. It is not without interest to remark, that, in excusing or accounting for faults and imperfections in the composition and language of the work, he expresses the hope that these imperfections may be excused, especially when it is considered that the finishing of this work was necessarily delayed till a very advanced period of the life of the author,—the seventy-seventh year of his age.\* It is somewhat singular, that, in assigning this reason, Dr Cullen was mistaken as to his own age in making it two years less than in truth it was. As he was born, according to the baptismal register of Hamilton, on the 15th day of April 1710, he was, at the time of the publication of the *Treatise on Materia Medica*, actually within three weeks of being in the seventy-ninth year of his age. If, therefore, advanced age was to be admitted as any excuse for imperfections

\* Preface, page xiv.

in composition and language, that excuse had a plea still stronger at the age of seventy-nine than at that of seventy-seven years.

Some persons had the want of good and generous feeling to turn this excuse against the author, and alleged that the work was that of a man who had outlived his best faculties, and in whom the vigour of the intellect was no longer perceptible.\* It is not

\* The remarks and criticisms now alluded to were not unfrequent in periodicals, and in other works. One example may suffice, derived from a work, the author of which has evinced commendable diligence, and, in general, impartiality and candour, in writing the History of the University of Edinburgh and its Professors; and it is therefore not creditable that in its pages is found the following statement.

“ Dr Cullen published a Treatise on the *Materia Medica* in two volumes, quarto, about a year before he died. (Eleven months.) A surreptitious copy of the Lectures he had delivered upon that subject, when unexpectedly called to finish what Dr Alston had begun, had been published. This was not designed to obviate the bad effects which might have been produced by that work. It was entirely a new Treatise. But the friends of Dr Cullen readily acknowledged that they perceived an evident decay in the mental energy of their great master. It is now quite unnecessary to conceal the truth. It was composed for the express purpose of raising a little money, and was thought to betray evident marks of a hasty compilation.”

The History of the University of Edinburgh, &c. By Alexander Bower. Volume Second, Chapter xix., p. 389.

This is simply an instance of misrepresentation, proceeding from one, who was giving an opinion on a subject, on which he was not qualified to judge. Who were the friends of Dr Cullen who acknowledged that they perceived an evident decay in the faculties of their master, as he is here styled, we are not informed. But, whoever they were, it is manifest that they

easy to say whether representations of this kind arose from envy and bad feeling, or positive ignorance and dulness, combined with a considerable amount of conceit and presumption. It might be proper, at least not unbecoming, in Dr Cullen to make the excuse to which we have referred. But that excuse refers entirely to faults and imperfections in composition and language, in no manner or sense to the matter of the work. In so far as concerns the substance of the work, the excuse was unnecessary ; and if it was in any sense to be taken into consideration, a mind imbued with proper feeling, and even with ordinary candour, must allow that the wonder was, that a man at the age of seventy-five years, and between that age and seventy-nine, was capable of producing a work so largely stored with new and well-founded facts,—a work in which so many erroneous doctrines were exploded, and so many absurd ideas were corrected,—a work

had neither studied the work from which they are supposed to have deduced their conclusion, nor knew any thing of the subject of which it treated. Certain it is, that if the faculties of the author were at that time in a state of decay, of this the work itself furnishes no proof.

One thing only we know, chiefly on the authority of Mr Paul. Cullen was in very low and depressed spirits during almost the whole time that he was engaged in the work. This was not unnatural, considering the loss of friends, which it has been shown he had sustained during the previous years, and the failing state of his own bodily health. If the Treatise is examined candidly on its own merits, without any reference to the circumstances under which it was written, it may safely be pronounced to be not only equal to the best of Cullen's writings, but by far the most practical and serviceable of the whole.

arranged in order so lucid, and one so thoroughly imbued with a spirit of just and philosophical reasoning. The Treatise on Materia Medica was not more remarkable for presenting an instructive view of the state of knowledge at the time at which it was published, than in being in many respects greatly in advance of these times, and in suggesting to physicians proper subjects for further inquiry, and more attentive and ample investigation. All those who were most capable of forming just opinions, agreed in recognizing the singular merits of the work ; and amidst a few murmurs, some censorious sneers, and some self-sufficient criticism, the voices of those whose judgment alone deserved estimation, were, if not unanimous, at least very generally agreed in overlooking slight and trivial errors, and recognizing the great superiority of the performance above all that had gone before it. It is not, however, upon its first appearance, or even after one or two perusals, that the true character of such a work can be estimated. The Treatise on Materia Medica is one of those works, the true value of which cannot be discovered at once. The subject itself is one so difficult, so complicated, so extensive, and, in the pursuit of which, the mind is beset with so many fallacies,—so many of what Bacon calls the *idola specus* and *idola theatri*, fancied resemblances, and imaginary qualities,—that it requires not only an intellect of superior power to examine it, but a mind of great acuteness, and well trained to inquiry, to follow these investigations when traced.

In order to form a just opinion of the merits of

Cullen's Treatise, it is indispensable to advert to the state of this department of Medicine both in this country and European countries in general, previous to and at the time at which Cullen delivered his prelections and composed his work.

To those who are acquainted with the history of science, and the progress and vicissitudes of human knowledge, it is scarcely necessary to say, that they have at different periods been largely vitiated by errors, absurdities, and falsehoods, the offspring of ignorance, indolence, and, not unfrequently, of superstition, knavery, and avarice. No department of human knowledge, having practical applications, however, has partaken of these evils in a greater degree than Medicine ; and of Medicine, no department has had so large a share as that called Therapeutics, and especially the *Materia Medica*, or that division which relates to the qualities, the uses, and the effects of Therapeutic agents and remedies, whether Dietetic or Medicinal. The causes of this it is unnecessary to investigate ; and if they were investigated, they would turn out to be little more than an historical illustration of human folly, ignorance, vanity, and superstition. In some instances many of the principal qualities ascribed to medicinal agents appear to have had their origin in the love of the marvellous and the mysterious. In others, they are manifestly the result of the desire of penetrating into what is beyond the reach of the human intellect, and pretending to knowledge on matters in which knowledge is unattainable. In other instances they are associated with such pursuits as magic, and judicial as-

trology, as in the case of the doctrine of Signatures. In not a few they are connected with chemical researches, especially the search for the *elixir vitæ*, the universal medicine, the philosopher's stone, and similar inquiries. The fact of this admixture and contamination of Materia Medica with the erroneous and pretended philosophy of the time must be admitted by all those who have, even in a cursory manner, directed their attention to this division of the healing art; and it becomes more distinctly manifest, the more we look through the writings which have been left, either by ancient authors of Greece and Rome, by the Arabians, or by physicians of the different countries of modern Europe.

At the time at which William Cullen undertook the duty of delivering Lectures on Materia Medica, that department of Medical Science was in a state which, without injustice, might be described as one of degradation, confusion, and inefficiency, with little science, little method, no reason, and void of precision. The discovery of the Caribbean Islands and the Continent of America at the end of the fifteenth century, had brought to the knowledge of European physicians several useful medicinal agents derived from the Vegetable Kingdom. That event and its subsequent results had made known such articles as Guaiacum, Zarza, Sassafras, Logwood, and, eventually, Jalap, Ipecacuanha, Cinchona, Seneka, Serpentaria, and similar agents. The travellers and adventurers, also, of the sixteenth and seventeenth centuries, had made us better acquainted with the medicinal products of

Asia and its divisions, India, Tartary, and China. Nor was the study of Chemistry, though in a state of great rudeness and inaccuracy, without its results in this matter. The art of preparing distilled spirits, which had been discovered by Arnold de Villa Nova in the fourteenth century, had been carried to considerable perfection in the fifteenth and sixteenth centuries. The art of preparing Ether had been taught by Valerius Cordus in his Pharmacopœia in 1545. The saline preparations of Mercury and Antimony had been gradually introduced as remedial agents, chiefly, indeed, by such men as Paracelsus, Leonhard Thurneysser, and Van Helmont ; yet, with such effect, that neither physicians nor surgeons could conveniently dispense with their employment. Lastly, the neutral salts were becoming daily better known and more commonly employed.

The effect of all this accumulation of stores and substances, which promised to extend and multiply the remedial means employed by physicians, had hitherto tended rather to encumber than to strengthen, and had certainly not increased in any perceptible degree the actual efficiency of the healing art. The state of *Materia Medica*, indeed, during the end of the seventeenth and the first half of the eighteenth century was remarkable for four circumstances. *First*, the lists of the *Materia Medica* were ample, and contained great numbers of articles which were more or less strongly and confidently recommended in the cure of diseases, and for the accomplishment of particular purposes in the economy. But they contained also many articles useless and

inert, and not a few to which were ascribed properties, which it was impossible they could possess. These lists, indeed, were exuberant and redundant to a fault ; and the effect of this redundancy was to perplex and confuse the ideas of practitioners, and render their practice rash, unsteady, irrational, and inefficient.

*Secondly*, the confidence reposed in the use of medicines of all the different classes was in general very great ; in some instances unbounded, in many irrational ; and it clearly appears, from the writings of physicians, that although many persons daily died of various morbid affections, nevertheless, physicians were in the habit of prescribing medicines and medicinal preparations, which they believed could always alleviate, and often cure, every known malady.

*Thirdly*, it was a common custom, both in the Pharmacopœias published by authority, and in the writings of individual physicians, to combine in one formula or prescription a great number of simple ingredients. All the Pharmacopœias and Formularies of the sixteenth and seventeenth centuries contain formulæ for various medicinal compositions, in which the number of ingredients is often very great,—varying in certain instances from twenty-four or twenty-five up to thirty-three, forty-eight, fifty, and fifty-two ; and formulas of this description are in not a few instances continued till the middle of the eighteenth century. Nor was it different in the writings of physicians. In the prescriptions left by Morton, and more especially in those given by several Continental physicians, we find the same fondness for



complicity of composition. This was much owing to the example of Fernel and Sir Theodore de Mayerne, the latter a person rated certainly at his full value, and a great lover and practitioner of complex prescriptions. In short, it was the practice at that time for physicians to crowd together numbers of differing, incongruous, often expensive, yet useless articles; and the great value of a prescription consisted in the number and multiplicity of articles which were directed to be mingled in it as ingredients. Sydenham is almost the first and only physician of that period in whose writings may be recognized a tendency to greater simplicity.

*Lastly*, In the practice of medicine, as generally conducted, there was no rational or logical connection between the disease or diseased states attempted to be cured, and the therapeutic means employed with the intention of removing them. A considerable proportion of the Therapeutic and Materia Medica department rested on empirical foundations. Not a few methods and remedies were suggested by superstitious observances. In few or no instances was it the custom, in laying down therapeutic methods and prescribing remedies, to study the nature and tendency of the morbid affections; and, in too many instances, the practical part of medicine was a system of blind and irrational administration of remedies, occasionally powerful, but unsuitable; sometimes inert and unserviceable, sometimes hurtful. The object of medical investigation in these days was not so much the inquiry into the nature of disease, the causes of its formation, and the means of pre-

venting it, as the incessant search for remedies and medicines, and the most agreeable method of administering them ; an object certainly laudable and useful when kept in its proper place, but liable, when carried, as it was at this time, to excess, to divert the mind of the physician from more necessary subjects of investigation.

Though this was the condition of Therapeutics and Materia Medica during the seventeenth and great part of the eighteenth century, so far as the great body of practitioners were concerned, it would be unjust, nevertheless, to infer that there were in the profession none who could carry their views to a more extended sphere, and reason in a more philosophical manner upon the treatment of diseases and the operation of medicines. We know, indeed, that during the course of the seventeenth century, there arose in England a race of men, mostly connected with one or other of the universities, who, with great industry, originality of thinking, and talent for investigation, made strenuous efforts to penetrate and understand the mysteries of the animal body, to elucidate the nature of various diseases, and to furnish at least the means of improving and rendering more efficient the treatment. Among the earliest of these was William Harvey, whose great discovery, though at first opposed, and rather slowly recognized, exerted at length an unquestionable and powerful influence. Contemporary with, or closely following him, came Thomas Wharton, Francis Glisson, Thomas Willis, Richard Lower, the pupil and associate of Willis, John Mayow, Nehemiah Grew, Walter

Charleton, Samuel Collins, Thomas Sydenham, Richard Morton, Christopher Bennet, and Henry Ridley.\*

It would be unseasonable in this place to notice even the comparative merits and services of these industrious and able inquirers. But it may be proper to remark, that while all of them studied anatomy, and dissected with different degrees of diligence and accuracy, Willis, Glisson, and Mayow, were active in applying their anatomical attainments in investigating physiology ; and all of them, but especially Willis, Glisson, and afterwards Morton and Ridley, availed themselves of every opportunity of elucidating the nature of diseases by the aid of morbid anatomy. Collins was the author of an excellent compilation on anatomy, human, comparative, and pathological, a work of great learning, and containing much useful information. Their endeavours in this respect were powerfully and effectually aided by the labours of various inquirers in France, Italy, Switzerland, the Netherlands, and Germany. When we consider, indeed, how many diligent observers arose, how sedulously and generally morbid anatomy was cultivated, and how much light was thrown on the nature of diseases during the seventeenth, and still more during the early part of the eighteenth

\* The names of these physicians are arranged as nearly as possible in chronological order. Thomas Wharton was born in 1610, and died in 1673 ; Willis was born in 1620 or 1621, died in 1677 ; F. Glisson was born in 1597, died in 1677 ; R. Lower, born in 1630, died 1691 ; J. Mayow, born 1645, died 1679 ; N. Grew, born in 1628, died in 1711.

century, it seems extraordinary that Therapeutics and Materia Medica derived from this source so little advantage, and continued so long in a state of comparative rudeness, unscientific, and of little efficiency. It is, nevertheless, not difficult to understand the reason of this backward condition of Materia Medica at that time. Though the facts calculated to throw light on the operation of medicines were numerous and instructive, they were not reduced into methodical order, nor arranged upon any scientific system. A long time must elapse before facts are generalized into principles, and it takes some time before principles can act with decided effect upon practices and methods which are firmly established, and have been sanctioned by long usage.

The idea, nevertheless, of applying the knowledge of Anatomy, Physiology, and Pathology, to the improvement and rectification of Therapeutics and Materia Medica had presented itself to the minds of physicians in the seventeenth century; and one attempt deserves particular mention. THOMAS WILLIS, who flourished between the middle and the third quarter of that century, and who, in the early part of his professional life, held a professorship at Oxford, had studied Anatomy, especially that of the nervous system, with great assiduity and attention; and his inquiries led him to speculate on various subjects in the pathology of the nervous system, on which his practice afforded him a considerable number of cases, several of which were accompanied by dissections. Willis had been led in this course of

inquiry not only to think much on the influence of the nervous system both in causing and in curing disease, but to reflect on the mode in which medicines act on the human body ; and with the activity of mind, which forms a prominent feature in his intellectual character, he undertook an extensive and rather elaborate inquiry into the actions and effects of medicines on the human frame.

In proposing to himself the duty of reforming the *Materia Medica* of the seventeenth century, Willis shows that he was quite aware of the degraded state into which it had fallen, and how far inferior was its condition to that of other branches of medical science. Medicines, he remarks, were prescribed promiscuously and at random, in every stage of disease, in every state of the patient. The pseudo-chemists boasted that they could prepare in their laboratories the great Elixir, the Panacea or Universal Medicine, and accordingly neglected the usual medicines, the properties of which were known. As to mineral remedies and medicines, of which the powers were either entirely unknown, or had no existence, or which were poisonous, they promised that they could with this new remedy, referring to antimony, immediately cure all diseases, and they exhibited it boldly in every case, to the great injury, and not rarely to the destruction of the patient. So heedlessly and rashly, he adds, are these executioners in the habit of sporting with the human body, while they are led to prepare and administer these dangerous medicines, not by any deliberation, or by the

guidance of any method, but by mere hazard and blind impulse.\*

Willis proposed at first in his investigation two objects ; to determine in what manner, and in what places, medicines and their particles act on the animal spirits, and in what and how many modes they cause fermentation in the blood and animal fluids. This fiction of animal spirits was an unfortunate prejudice,—a fanciful creation,—of the age. As to the fermentation, it was a sort of figurative mode of speaking about the blood and fluids and their internal motions within the body, which was common to Willis and others of that time, and on which he had written an express treatise. Notwithstanding these fanciful and unfounded assumptions, however, Willis perceived that it was indispensable, as a preliminary condition, to consider the minute structure and the principal properties of the different textures and organs, and to make known, as clearly and correctly as possible, all that could be learned of the different organs by means of anatomy. In this inquiry, he acknowledges the aid which he received from Dr Edmund King and D. Master. His frequent recurrence to these anatomical inquiries shows the importance which he attached to anatomical knowledge in explaining the action of remedies.

His Treatise, to which he gives the name of PHARMACEUTIKE RATIONALIS, consists of two parts.

In the first he commences with the description of the Alimentary Canal, and gives an account of the

\* Pharmaceutike Rationalis. London, 1675, Præfatio.

minute structure of the Œsophagus, the Stomach and Intestines, the Gall Duct, the Pancreatic Duct, and, more shortly, the Mesenteric Arteries and Veins;\* and in a subsequent section he treats of the Structure of the Arteries in general.† After describing the structure of the Alimentary Canal, he treats of the operations, first, of Emetics; secondly, of Cathartics; thirdly, of Diuretics; fourthly, of Sweating and Diaphoretics; fifthly, of Cordials and Alexiterics; and, lastly, of Hypnotics and Opiates. In the course of this Treatise, Willis communicates a large amount of valuable information on various important diseases at that time or previously prevalent; on Dysentery, especially that of London in 1670; on Diuresis and Diabetes; on the Sweating Sickness; and on certain affections of the Heart.

In the second part of his Treatise, Willis explains in some detail the structure of the Lungs and Bronchial Tubes, the course of the Pulmonary Circulation, the agents and mechanism of Respiration. He then treats shortly of the various forms of Morbid Respiration, their causes and symptoms, communicating under this head a large amount of interesting information on Phthisis and its causes; on Hæmoptysis, Pneumonia, Pleurisy, Empyema, Vomica, Asthma, and Hydrothorax; and at the same time giving the appropriate therapeutic indications, and the remedies and medicinal compositions by which these are to be fulfilled. In a second section of this Part,

\* *Pharmaceutike Rationalis*, Sectio I., Caput II.

† *Ib.*, Sectio VI., Caput II.

he treats of diseases of the Abdominal Organs, as Jaundice, Askites, Tympanites, Anasarka, and the remedies and medicines suited to the alleviation and cure of each disorder. In a third section of this Part, he treats of Hemorrhage, and the causes and effects of that discharge, and also of Phlebotomy, and of remedies required for suppressing hemorrhages. He next considers the effects of Vesicatories and Issues ; and, lastly, he speaks shortly of Cancer and its remedies, and of Cutaneous diseases and the remedies required for their removal.

Willis allows that his Treatise on Rational Pharmaceutics is incomplete, and does not come up to the speculative idea which he had originally formed of what was requisite to be accomplished. In the latter part of his performance, though he never loses sight of the main object of Materia Medica and the adaptation of remedies to morbid states, yet the treatise insensibly assumes the character and appearance of an essay on Special Therapeutics or Practice of Medicine, rather than one on Materia Medica. This is mentioned not as an objection to, or a censure upon the Treatise, but merely to show the difficulty, in a first attempt, of carrying into complete effect the plans and conceptions first suggested by the necessities of the case.\*

\* The Second Part of Pharmaceutike Rationalis was posthumous. The *Imprimatur* is dated 12th November 1675. Oxford.

Dr Willis's Practice of Physic: Being the whole works of that renowned and famous physician, &c. &c. The Pharmaceutike newly translated, and the whole carefully corrected and amended. London, 1684. Folio. Most of this translation is by S. Pordage.



The conception of the PHARMAKEUTIKE RATIONALIS was undoubtedly excellent, and shows how philosophically Willis desired to treat the subjects of Materia Medica and Pharmacy. His object was to establish between the physiological properties and the morbid conditions of the human body on the one hand, and the curative indications, and the means by which they were to be fulfilled, on the other,—a direct and rational connection; to make the therapeutic measures flow as directly as possible from the conditions, especially the morbid conditions, of the human body, and the powers which it naturally possesses of resisting the progress and action of morbid agencies. It will not excite the wonder of any one who knows the true difficulty of the task, to learn that in this undertaking the success of the author was only partial. The treatise is most valuable and instructive in many respects,—in its anatomical information, in the incidental notices of various important diseases, in the complete description and illustration by cases of many other diseases, and it may at all times be consulted and studied with profit by the physician. But the portion of it directly devoted to the subject of Materia Medica and Pharmacy, though better than perhaps it would have been had it been the work of any other person, was by no means such as to make any decided improvement in the doctrines and practices of the day. I feel the difficulty, I may say the impropriety, of speaking critically and judging severely of the writings of one, whose diligence as an observer appears so clearly in all that he has done as Willis. But I confess that it appears to me extraordinary, that such

a man should retain in his lists such pretended medicines as Blood, the Human Skull, Pearls, Salt of Vipers, Water of Snails and Earthworms, Millepeds, and similar articles, which could have no effect upon any disease. It must in truth be acknowledged, that Willis gave his confidence where he ought to have withheld it; that he should have retrenched with a hand judicious, if not unsparing; and that if he had doubted where he believed, he would have rendered much service to the science and benefit to the art. In addition to this credulousness and excessive confidence in the powers of useless articles, there may be observed undue confidence in the powers of medicines in general. In the selection of remedies for particular diseases, we observe not the discrimination, which it was reasonable to expect from one so well versed in morbid anatomy, and an amount of confidence in agents of doubtful properties, or without properties, for which it is not possible to account. In the Formulæ is observed the same multiplicity of ingredients for which the time was remarkable.

It is more important and useful, as it is more easy, to notice the facts of this imperfect success, than to attempt to suggest for them any explanation. But it is probably just to observe, that the errors, defects, and shortcomings of the attempt of Willis seem to belong very much to the age in which he lived, to the prejudices inspired by education, to the habits of thought and modes of reasoning on medical subjects then in fashion. They show, indeed, how difficult it is for one individual, however enlightened, to demonstrate and eradicate all the errors which

are prevalent, and have been long established in the minds and habits of society. One conclusion, it appears to me, manifestly results from examining the *Formulæ* given by Willis, and from reading his account of their occasional effects. It seems clear, that several, if not many of the remedies, could have exercised little or no effect on any disease, and that either the diseases in question were cured by some effort of nature, that is by the powers of the system itself, or that the symptoms disappeared under the influence of the imagination of the patient and the confident belief that the remedies prescribed exerted, and ought to exert, a certain degree of efficacy and therapeutic agency.

It must be remembered, also, that the attempt of Willis was not only the first, but, according to his statement and belief, the only one of the kind that had been made. It is not difficult to perceive that no great progress could have been expected to be made in overthrowing long-established errors, and establishing a system even partially new.

While Thomas Willis was engaged in reforming the science of Pharmacology in England, an attempt of the same nature was made by two German physicians. JOHN ZWELFER, a learned physician of Vienna, had published, in the year 1651, a new and amended edition of the Augsburg *Pharmakopœia*, under the title of *PHARMAKOPŒIA AUGUSTANA REFORMATATA*. This performance, which was the result, Zwelfer states, of the labour of sixteen years, in examining, correcting, and improving the *Formulæ* of the Augsburg *Pharmakopœia*, was a considerable improvement upon all the previous editions. Re-

trenchment of superfluities, selection of what was good, rejection of what was useless and improper, and general improvement of *Formulae*, were the principal objects of Zwelfer ; and the work had the effect of causing the medical practitioners of Germany to reflect, to think, and to inquire. In 1652, the same author published, at Vienna, a sort of Dispensatory of his own, which was still more decidedly marked by original suggestions, salutary abbreviations, and useful changes ; and as a new edition of this was found necessary in 1666, it may be inferred that the work was studied, and that its instructions were not without effect.

Among those whose minds were thus directed, was DANIEL LUDWIG,\* a native of Weimar in Saxony, Archiater or Chief Physician to the Principality or Dukedom of Saxe-Gotha, who had for some time beheld the unsatisfactory and inefficient state of *Materia Medica*, in consequence of the luxuriance, exuberance, and redundance, of the lists of medicines and medicinal preparations and compounds. He accordingly published, in 1671, first a Dissertation chiefly on this evil, in which he maintained that the lists of all kinds of medicines were so numerous, yet useless, that the Practice of Medicine might most advantageously be conducted by means of one-twelfth or one-twentieth of the number. He consequently strongly recommended retrenchment and selection, and in general the principle of taking the most

\* Born at Weimar in 1625, died in 1680, it is said of *Marasmus Senilis* ; (*Marcor Senilis*).

easily procurable agents. It was at this time, namely in 1671, and previously, the fashion to prepare from various vegetable, animal, and mineral substances, what were called *Magisteria*, which were supposed to contain the essential virtues of the articles from which they were prepared. Against these *Magisteria*, therefore, the number of which was very great, Ludwig waged war as against articles useless, expensive, and altogether unsuitable for the cure of disease. In the same manner he pronounced a decided condemnation on the essential salts of vegetables, on the volatile salts of the animal kingdom (ammonia), on Elæosacchara, and on the whole tribe of confecti-  
tions and conserves. In the same spirit, Ludwig questioned the utility of the numerous masses of purgative Pills, Electuaries, Loochs or Linctuses, distilled or essential Oils, Ointments, Plasters, and Cerates ; and, lastly, of the multiplied forms in which the Metals were then disguised, or pretended to be disguised. He afterwards enters more in detail upon the consideration of the respective and actual utility of Purgative Medicines from the Vegetable and Mineral kingdom ; Diaphoretics from the Vegetable, the Animal, and the Mineral kingdom ; Diuretics from these sources ; the medicines designated Aperient, Incident, Detergent, Splenetic, Hepatic, Uterine, Pectoral, and maintains that, for the purposes so contemplated, either the medicines were too numerous, or they were entirely inert and inefficient, and, in short, entirely unworthy of the characters assigned to them. He is not more indulgent to alleged Resolvent and Roborant Medicines ; to Ce-

phalic and Nervine agents ; to Hypnotics, Anodynes, and Epikerastics ; and, finally, to Vulnerary, Astringent, and other external applications. Ludwig appears to have been aware, that all these denominations were applied without any good reason, and that the properties and qualities which they were intended to designate were altogether imaginary and arbitrary. It is quite unnecessary to follow him in his discussion of this subject, and it is not wonderful that of the Millepedes or Slaters, Earthworms, Toads, and similar articles, which were then used as diuretics and lithontriptics, he entirely denies the virtues, and pronounces all of them, in whatever mode prepared, to be abominable, and altogether unworthy of credit. The Dissertation is prolix, and somewhat tedious. The whole substance of what he says might have been stated in two or three pages. The task which he undertook was not one of great difficulty ; and what he said was the expression of the sentiments of sensible men on that compound of folly, ignorance, credulity, and extravagance, the Pharmacological Lists of the seventeenth century.

In a Second Dissertation, Ludwig treats of the necessity of moderating the rates at which medicines were sold ; and, in a Third, on the private dispensing of Remedies by physicians. Of these we need say nothing.

The Dissertation of Ludwig was highly esteemed by the physicians of Germany. They speak of it in strong terms of commendation ; and to its influence they ascribe great amendment in the state of Materia Medica and Pharmacy. He appears to have

been a person of considerable learning, sound judgment, and some originality in thinking. But he is much inferior to Willis, both as a physiologist and as a scientific and philosophical inquirer. The Dissertations were several times republished; and, latterly, in 1712, by John Conrad Michaelis of Hanow, with a commendatory preface.\*

Notwithstanding all the defects, all the imperfections of the Treatise of Willis, it is scarcely possible to doubt that his example, his reasonings, and his method of treating the subject, must have exerted in process of time a considerable influence upon the state of *Materia Medica*. Most of his practical writings were translated into English, and thus became better known in his own country;† while abroad, the Latin original appears to have been well known and highly esteemed. Their influence may be recognized in the Pharmacological works which appeared towards the end of the seventeenth century and the beginning of the eighteenth; less clearly in the work of Moses Charas, a native of Languedoc, practising Chemistry and Pharmacy at Paris,‡ and in the better known writings of Nicolas Lemery,§

\* D. Danielis Ludovici Opera Omnia. Opera et Studio Johannis Conradi Michaelis, M.D., &c. Francofurti ad Moenum, 1712.

† Dr Willis's Practice of Physic. Being his whole Works translated, &c. &c. Folio. London, 1684.

‡ Pharmacopée Royale Galenique et Chymique. Par Moyse Charas, à present D. M. et Chymiste du Roi de La Grande Bretagne. 1<sup>ere</sup> Edit. 1676. Seconde Edition. A Paris, 1682. 8vo. Deux Tomes.

§ Pharmacopée Universelle, Contenant Toutes les Compo-

than in the last work of the same person, the Universal Dictionary of Drugs,\* published at Paris in 1698, and after his death in 1748. Lemery had evidently begun his professional life with great confidence in the powers of medicines and medicinal compositions. But as judgment was matured, as experience became extended, his estimate of the influence of medicines appears to have become more moderate. That he proceeded with great caution in reforming, appears from the statement which he makes, and for which he takes credit, that from the sixty-two articles which entered into the composition of the Mithridate, he retrenched twelve. Had the retrenchment amounted to fifty-five or fifty-six, the claim would have been better founded. But amidst long-continued and deep-rooted prejudices, the progress to truth and common sense is slow and difficult. That Lemery, nevertheless, was aware of the vicious redundancy of the Materia Medica of his time seems undoubted. But the author of the biographical account, in the Paris Transactions, states as distinctly as he can be expected to do, that Le-

sitions de Pharmacie, qui sont en usage dans la Medecine, tant en France que par toute l'Europe, &c. Par Nicolas Lemery, D.M. A Paris, 1698.

Traité Universelle des Drogues Simples mises en Ordre Alphabetique. Ouvrage dependant de la Pharmacopée Universelle. Par Nicolas Lemery, D.M. Paris, 1699. 4to.

\* Dictionnaire Universel des Drogues Simples, Contenant leurs Noms, Origine, Choix, Principes, Vertus, Etymologie, &c. Ouvrage dependant de la Pharmacopée Universelle. Par Feu. M. Lemery de l'Acad. Royale des Sciences. Troisieme Edition. A Paris, 1748.



mery complied with the spirit of the times in retaining many of these articles in his lists. This writer pronounces on these lists an opinion which probably was not peculiar to himself, and merely expressed the sentiments of various well-informed physicians.

“The immense mass of remedies, either simple or compound, contained in the Pharmacopœia, or in the Treatise on Drugs, would seem to promise immortality, or at least a certain cure for each disease. But it is with these as it is in society, where we receive a considerable amount of offers of service, but very few real services. In this crowd of remedies we have few real friends. M. Lemery, who knew them all, trusted only to a small number. He employed chemical remedies only with the greatest circumspection, though he might be naturally disposed in their favour, and encouraged by this predisposition, which takes possession of many minds. Almost all the analyses he gave only to the curiosity of physical inquirers; and he thought that, in reference to medicine, chemistry, by reducing compounds to their principles, reduced them often to nothing; that a day would come in which it would adopt a contrary route, and instead of decomposing, it would become compounding, that is to say, it would form new and better remedies by combining different compounds. Those that are most able in any profession are not those who boast most of its powers. They are above this.”\*

The Complete History of Drugs by Pomet, though

\* Eloge de M. Lemery. Histoire de l'Academie des Sciences, année 1715. 4to. Paris, 1718. P. 73 et p. 80.

written after the authority of Lemery, and translated into English in 1712, is little else than a repetition, not very useful, of all the matter given by his predecessor.\*

The limits within which these notices must be confined, allow me to do no more than mention the names of such writers as George Wolfgang Wedel, Hermann Nicolas Grimm, Andrew Cleyer, and the industrious compilers of the *Hortus Malabaricus*, Commelyn, John Munniks, and their associates.

The progress of truth and common sense was indeed slow; sometimes impeded by considerable obstacles; sometimes entirely stationary. At no time, perhaps, was there absolute indolence. But labour, thought, and experiment were often wasted and misapplied. Some advancement, nevertheless, was made, and various circumstances contributed to direct the minds of medical practitioners in more judicious paths, and to more useful subjects than previously. The establishment of the Royal Society in London, in the reign of Charles the Second,† and the institution of the Academy of Sciences in Paris in 1666, exerted some influence, in this respect that their meetings and their Transactions afforded the means of collecting useful information from all quarters,

\* A Compleat History of Drugs, written in French by M. Pomet, Chief Druggist to the present French King; to which is added what is further observable on the same subject from Messrs Lemery and Tournefort. Done into English from the Originals. Two Volumes 4to. London, 1712.

† The first origin of the Royal Society was in 1645, at Oxford. It received the Royal Charter on the 15th July 1662, and began to publish Transactions in 1665.

and upon many subjects, and recording whatever deserved to be known. There was, in short, great activity and zeal ; and if knowledge was not in all instances actually acquired, and was obtained only after various errors had been committed, still, inquirers were put in the way of making some acquisitions. The knowledge of the powers of Cinchona had been introduced into Europe in the year 1638 ; and though for many years the remedy encountered great opposition, and struggled against many difficulties, yet it acquired, towards the close of the century, a firm hold of the opinions of physicians, and the use of the remedy was of service in directing their minds to simple and natural methods of treatment. The use of Tea and Coffee, which was commenced about 1678 and 1679,\* at first as medicinal articles and luxuries, and afterwards as dietetic drinks, may have had some effect in diminishing the use of the numerous vegetable infusions then employed as Ptisans, and probably in diminishing the use of intoxicating drinks. It is at all events certain, that, upon these three articles, namely, Peruvian Bark, Tea, and Coffee, an immense number of essays and treatises appeared during the close of the seventeenth and the first third of the eighteenth century, some against, many in favour of them.† Cacao and Chocolate also had

\* The use of Coffee was introduced into France by Thevenot in 1671, or, according to some, in 1669. Tea, according to the author of *Hortus Elthamensis* ; in 1687.

† Neumann, in 1735, enumerates thirty-five writings, mostly monographs, on Tea ; of these, thirteen on surrogates for Tea ; —besides incidental notices in other works. The same author

many advocates. Tobacco had friends and opponents.

Among those who studied to amend and extend the knowledge of *Materia Medica* at this time in England, it might be regarded as an omission not to mention John Ray and the Honourable Robert Boyle.

John Ray\* studied natural history, and especially botany, with great attention and acuteness, and possessing a mind of much accuracy, and a great talent for distinction and classification, he was enabled to show the advantages, and lay the foundation of that method of arrangement which long afterwards became so serviceable in the hands of Linnæus and his pupils, and in the writings and works of De Jussieu, Persoon, Bonpland, and Decandolle, and those of Sauvages, Sagar, Vogel, and Cullen. Ray was not himself a physician, but he lived in close intimacy with several of the most eminent physicians of his time, — Sydenham, Tancred Robinson, Sir Hans Sloane, and Sir Thomas Millington. The information which he introduced into his writings on the medical properties of Plants was principally derived from friends and correspondents, whose testimony was not entirely free from question. In his own department, he was evidently a person of great judgment, and spared no pains to arrive at truth. But in assigning

enumerates twenty-seven writings, mostly monographs, on Coffee; but in several of these, about eleven or twelve, Tea is also considered. Not a few dissertations on both are evidently omitted.

\* Born in 1628; died in 1705.

the medicinal properties which he does, he is often credulous and incautious ; and that an inquirer like Ray has done so, is a proof of the great difficulty of avoiding error and attaining accuracy in inquiries of this nature ; or it shows that physicians in general were at that time lax, credulous, and incautious in receiving statements regarding the medicinal properties of the agents which they employed. John Ray was a great invalid in the latter part of his life ; and it is painful to observe the frequent attacks of severe illness upon a chronic state of imperfect health under which he laboured, and the frequent and anxious trial of remedies, almost always useless, by which he studied to alleviate his sufferings.

The Honourable Robert Boyle\* was a person of great zeal and mental activity ; and among the multifarious subjects which occupied his attention, the question of the efficacy of different medicines and methods of treatment was not the least important. Boyle was attached to what is called the Corpuscular Philosophy, and his great purpose is to reconcile to the principles of this doctrine the powers and operations of medicines ; (1685). He was a believer in specific medicines, and sought to discover what were specifics ; an inquiry which might be useful, if in the course of that or in any other mode, the existence of specifics was established on irrefragable grounds. On the other hand, he was an advocate for the employment of simple remedies. It is, nevertheless, a circumstance much to be regretted, in the

\* Born in 1626-7 ; died 1691.

otherwise estimable character of this benevolent person, that he was credulous, and apt to be imposed upon ; that the reports of the effects of remedies which he published are too favourable ; and that he believed in the efficacy of periapts and amulets.

Most of the writings of Boyle were published between the years 1661 and 1690. But two of his treatises, the object of which was to ascertain and make known the effects of remedies, were published in 1685 and 1688-92 respectively. It is worthy of remark, that Locke's Essay on the Human Understanding, which taught so much of the natural history of the human mind, and the laws of reasoning, was published in 1690 ; and that the first edition of the PRINCIPIA of Newton, which gave the finest example of the true and just method of investigation, appeared in 1687, and the second in 1726, while the Optics appeared in 1706. Yet the instructions of the former work, and the example of the latter, exerted scarcely any appreciable influence upon the reasonings of physicians at the commencement of the eighteenth century.

Of Jean Baptiste Chomel, I need say nothing. Cullen has given a character of his work, which it is not necessary to repeat. It is enough to say, that though he has not imitated several of the errors of his predecessors, he has not in any remarkable degree amended or improved the Materia Medica of his day.

Stephen Francis Geoffroy\* succeeded first De

\* Born 1672 ; died 1731.

Tournefort as Professor of Medicine and Botany at the Royal College of France in 1709, and afterwards, in 1712, M. Fagon, as Professor of Chemistry. His Lectures on *Materia Medica* were published in 1741, ten years after his death, in an unfinished state. These lectures contain the history of the Mineral and the Vegetable substances employed in the Treatment of Diseases ; and a Supplement was afterwards published, intended to complete the work. Geoffroy was a person of great learning and information ; and in his work, the Natural and Commercial History of the articles is delivered fully, and, in general, as correctly as information at that time allowed. The Chemical accounts, also, are supposed to be the best at that time attainable. But it is a serious objection to his work, that, like most of his predecessors, he admits, as medicinal agents, many substances upon most insufficient grounds ; that he is credulous and incautious in receiving the statements of previous authors as to the virtues of many agents ; that he often evinces little judgment in the adaptation of remedies to diseases ; and that his physiological and pathological comments and explanations, where they occur, are trifling, fanciful, and hypothetical. Several statements regarding the virtues of Plants he takes from Ray and his English authorities ; and several of these are silly enough. But his easy and slavish adoption of the statements of Simon Pauli, a writer void of judgment and penetration, is an instance of the difficulty which ordinary minds experience in exerting any independent power of thought and reasoning.

I am uncertain whether in this place I should mention the labours of Caspar Neumann, a native of Zullichan, and afterwards Professor of Chemistry at Berlin. He receives no place in the History by Cullen, I presume, because his researches were believed to be devoted to Chemistry rather than to Materia Medica. This must be admitted ; but it must also be observed, that Caspar Neumann was an assiduous inquirer into the chemical properties of many articles employed as medicines. He communicated papers to the Royal Society of London, to the *Actâ Physico-Medica* of Vienna, and to the *Miscellanea* of Berlin, and he published various separate tracts and writings on several substances in more or less general use, either as medicines or dietetic articles. Thus he wrote on Camphor, on the Fixed Alkalis, on Ambergris, on the Oil of Ants ; on Amber, Opium, Castor ; on Saltpetre, Sulphur, Antimony, and Iron ; on Tea, Coffee, Beer, and Wine ; on common Salt, Cream of Tartar, Sal-Ammoniac, and the Ant.\*

\* *Lectiones Publicæ von Vier subjectis Chemicis, nehmlich vom Salpeter, Schwefel, Spiess-Glas und Eisen.* Von D. Caspar Neumann, *Chemiæ Pract. Prof. P. u. s. w.* Berlin, 1732. 4to.

*Lectiones Publicæ von Vier subjectis Dieteticis, nehmlich von dem Viererley Getrancken, vom Thee, Caffée, Bier, und Wein.* Von D. Caspar Neumann, Königl.-Preussischen Hoff-Rath und Profess. Publ. *Chemiæ Pract.* Leipzig, 1735. 4to.

*Lectiones Publicæ von Vier subjectis Pharmaceutico-Chemicis, nehmlich vom Gemeinem Salze, Weinstein, Salmiac und der Ameise ; (the Emmet or Ant).* Von D. Caspar Neumann, Königl.-Preussischen Hoff-Rath und Professor Publ. *Chemiæ Practicæ.* Leipzig, 1737. 4to.



In writing on so many subjects, it was not to be expected that the information was, in all instances, considering the state of Chemistry, to be correct and free from errors. But the researches which Neumann undertook, and the results which he occasionally obtained, had an effect not inconsiderable in directing the attention of physicians to the chemical constitution and the physiological effects of the substances which they employed. His observations on Beer and Wine are allowed to contain much useful information, which at that time was also new ; and his Analysis and Analytical Statement of the Proportion of Alcohol in different species of Wine, have been thought sufficiently near the truth to be quoted in comparison with the modern researches by Brande, Prout, and Julia-Fontenelle. Of his writings, an abridged translation was published in 1759 by Dr William Lewis, who also reduced the whole into more methodical order, and added, from his own information, and that of his contemporaries, much that tended to improve the work, and render it useful to the physician.\*

Neumann probably overrated the utility of chemical analysis in elucidating the physiological and therapeutic effects of medicines, and the hygienic effects of dietetic articles. Such at least was the opinion of Cullen in reference to chemical analysis in general ; and it must be allowed, that though

\* The Chemical Works of Caspar Neumann, M.D., Prof. of Chemistry at Berlin. Abridged and Methodized with large Additions, containing the later Discoveries and Improvements made in Chemistry and the Arts depending thereon. By William Lewis, M.B., and F.R.S.. London, 1759. 4to.

chemical analysis was at that time in a very rude condition, and little capable of furnishing serviceable results ; yet, even with the most accurate chemical knowledge of the present day, it is quite impossible to form just ideas on these points without having recourse to accurate observation and experience. It must at the same time be admitted, that without as correct knowledge as possible of the chemical constitution both of medicines and dietetic articles, the physiological and medical reasoner must be much more in the dark, than if possessing this knowledge. If it were becoming to speak with decision on a question, on which the most able men have entertained opposite opinions, it might be said, that, in proportion as chemical analysis becomes more accurate, and furnishes more complete information on the chemical constitution of medicines and dietetic articles, in the same rate it will enable the medical reasoner to perceive more clearly, and understand more fully, the action of medicinal and dietetic agents upon the animal body.

It is unfortunate, nevertheless, that the chemical and medical knowledge of Neumann did not save him from placing confidence in various useless and antiquated articles ;—\* errors for which he is justly censured by Dr Lewis.

One of the great evils of *Materia Medica* and *Pharmacology* during the first half of the eighteenth century, was the state of the *Pharmacopœias* pub-

\* He recommends as medicines, Powder of Human Skull, Millipeds, Oil and Acid of Ants, and similar articles.

lished by authority of the Colleges and Medical Incorporations; and as these productions were in some sense exponents of the collective judgment and Pharmacological knowledge of the profession, and were understood to be guides to the great body of medical practitioners and dispensers, it seems necessary to advert shortly to the general characters of the two Pharmacopœias published by the London and Edinburgh Colleges.

On the 2d of September 1720, the sanction of the Royal College of Physicians of London was given to the publication of a new edition of the Pharmacopœia, which appears to have been the sixth.\*

But though this appeared under the presidency of Sir Hans Sloane, and while Mead and Freind were among the censors of the College, the work retains all the faults, imperfections, and errors of the time. Little, almost nothing, is effected in retrenching superfluities and restoring simplicity either in medicines or in medical compositions. Many useless articles and formularies are allowed to stand; and the luxuriance and multiplicity of composition, so dear to the old receipt books, is seen in unabated vigour. The noted standing compositions of the Theriaca and Mithridate, with the confection of Damocrates, occupy their wonted places; and along with them is placed another compound of the same stamp, namely, the *Confectio Raleighana*. The only change of moment is, that the Electuaries are distinguished into Alterative and Purgative; and, con-

\* The Preface presents the date 30th September 1720.

sequently, this section appears less lengthened and less confused than in the early editions.\* It is altogether astonishing that the College could sanction the appearance of such an amount of folly and absurdity, at a time when the polished wisdom of Addison had been long enlightening his countrymen, while Pope, Swift, and Arbuthnot were in existence, and Dr James Douglas and Cheselden were teaching Anatomy. But Arbuthnot, though a physician, seems not to have taken very profound views, at least in Pathology, whatever was his strength as a wit and elegant writer.

Nearly the half of the eighteenth century had elapsed before the College began to think that the Pharmacopœia contained many things which ought to be discarded, and was destitute of several which ought to be introduced. A Committee of Revision had been appointed, and, in the year 1742, they presented an interim Report on the actual state of the Pharmacopœia, with a draft containing sundry proposed changes to be introduced in publishing a new Pharmacopœia. This Report is the first example of sound thinking, and just views applied to the business of preparing a Formulary. The reporters admit the superfluity of Formulæ, the complication in their construction, the great numbers of useless simples and medicines encumbering the lists, the improper and unscientific preparation of some medicines, the dangerous character of some of the

\* Pharmacopœia Collegii Regalis Medicorum Londinensis. Londini, MDCCXXI. Folio. Pp. 154. Et 1724, 12mo. Pp. 176.

antimonials, the fatal effects resulting from the irregular preparation and dispensation of Cherry Laurel Water, the luxuriant and redundant composition of the syrups and electuaries, the uselessness of many of the ointments and plasters, and various errors in nomenclature as well as irregularity in the denominations of weights and measures. They allow, that though the writings of physicians had become more rational and philosophical, yet all the Pharmacopœias in Europe, either from inattention or too great reverence for preceding times, still remained encumbered with operose and incoherent compositions. Even the new forms, they remark, which have from time to time been introduced, often prove to be rather innovations than amendments,—old directions under new aspects, or they have usually been the invention of private individuals, and transcribed into public Pharmacopœias from the original receipts, without the requisite examination, before they should have received so solemn and authoritative recognition.

In 1745, the same committee presented another report, which was to be final; and upon their recommendation the New Pharmacopœia was constructed. Much was done in the way of simplification, in retrenching superfluities, and lopping off what was redundant. All the Formulæ were shortened and amended. The whole class of Loochs or Linctuses was discarded. Matthew's Pills,\* a sort

\* Consisting of Castor, Saffron, Opium, Soap, and Balsam of Copaiba. They were originally composed by a quack named Starkey.

of patent sedative medicine, were ejected. The *Emplastrum Sticticum*, or German Stick-plaster, not sticking plaster, but wound plaster, or pain plaster, was also dismissed. Many ointments and plasters, in like manner, were discarded.

Several unnecessary and not very judicious articles, nevertheless, are retained. Besides the Mithridates and Theriacs, which appeared to have gotten an immoveable hold of the Pharmacopœias, there were Viper Wine and Viper Broth, *Pulvis Bezoardicus*, Amber Lozenges, and Brick Oil, prepared by immersing heated bricks in oil, then breaking the bricks in pieces, and distilling the fragments in a retort. The *Confectio Raleighana*, simplified and curtailed, appears under the name of *Confectio Cardiaca*,\* or Cordial Confection.

Such was the state of Pharmacology in England and its metropolis in 1745–1746. In Scotland it was not better; perhaps a degree worse. The Pharmacopœia of the Edinburgh College, published in 1744, which is the fourth edition of that performance, may be regarded as a just example of all the faults and absurdities of the old formularies. In the preface, it is stated that the great variety of medicines, and the increase and progress of the art of Pharmacology, had caused great vicissitudes in that department. The result of this was, that in the Edinburgh Pharmacopœia were wanting many articles which practice had introduced; and though the book was in all points more contracted than in previous edi-

\* Pharmacopœia Londinensis. Londini, 1746.

tions, yet not a few substances were found quite obsolete. The College, accordingly, having a view to the advantage of the public and its own dignity, had issued this new edition, corrected and enlarged, to be a standard and rule to all apothecaries in the city in compounding medicines.

The Materia Medica List or Catalogue, which they denominate sufficiently ample, is not only luxuriant and redundant, but contains many articles useless and altogether absurd. Of the long and tedious list of plants, I shall say nothing. But a few of the articles from the Animal Kingdom may be properly noticed. Here we find the fat of the Goose, Duck, and Man ; Cobweb ; the Toad, entire ; the Spawn of the Frog ; Ants entire, and Ant Eggs ; the Human Skull and Mummy ; Snails, Earthworms, Millepeds or Slaters ; Pearls and Mother of Pearl, *Canis Stercus* ; the Scorpion and Viper. The list of minerals is not less luxuriant and foolish.

In the Formulary, we find *Tinctura Salutifera* formed from nine plants ; the *Tinctura ad Stomachicos* from nine plants and one metal ; and *Vinum Millepedatum*, formed by pouring one pint of Rhenish wine upon two ounces of these loathsome insects still living.

The Electuaries, however, and the Confections, were the great resorts of complication and multiplicity, of folly and extravagance in the old Pharmacopœias ; and here the Edinburgh one is not behind its contemporaries. Here stands the Mithridate, or Confection of Damocrates, consisting of forty-seven ingredients, with a proportion of Canary Wine ; and

the Theriac of Andromachus, consisting of sixty-four ingredients with Canary Wine. But as if the College were not satisfied that these two compounds would answer the views of all prescribers, they give one of their own, consisting of ten ingredients with Canary Wine, and Camphor to be added according to circumstances. The same thing had been done before by the London College.

Two of the Lozenge tribe, the *Trochisci Cypheos pro Mithridatio*, and the *Trochisci pro Theriaca Andromachi*, are only a little less luxuriant, not less foolish.

Lastly, the *Emplastrum Defensivum*, consists of about ten or eleven vegetable articles, three mineral substances, with hog's lard, yellow wax, white resin, olibanum, and Venice turpentine.

I shall merely remark, that among the list of the Fellows of the College at this time, we see the names of John Rutherford, Professor of Practice of Medicine; Charles Alston, Professor of Botany; Andrew Plummer, Professor of Chemistry; Sir John Pringle, and Robert Whytt, afterwards Professor of the Institutes of Medicine.

It seems impossible to doubt that some of the fellows now named, and perhaps various members subsequently added to the College, must have become aware of the unbecoming appearance and unscientific character of this fourth edition. The composition and character of its prescriptions and formulæ were certainly little in accordance with the improving and simplifying tendency of rational physicians. Sir John Pringle had by this time set an example of



simplicity in prescription, which was appreciated and imitated. Huxham, with not less judgment, had\* expressed his sentiments in the following terms:—

“ I have given few or no *Formulae* or *Prescriptions*; for as *Hippocrates* says, he that knows the disease, knows what is proper to cure it. When a physician knows whether Stimulants or Anodynes, Relaxants or Restringtons, Attenuants or Incrassants, are indicated, he can be at no great loss how to serve himself of proper drugs out of the vast *Materia Medica* which we at present abound with. He should select a few of the most effectual for his Use of each Sort, and stick to them, and not run into that immense *Farrago* which some are so fond of. By so doing, he will soon be acquainted with their real Virtues and Effects, and readily distinguish between the symptoms of the disease and those caused by the medicines, which is a thing many times of no small importance. I have really seen in private practice, and some public writings, such a jumble of Things thrown together in one Prescription, that it would have puzzled *Apollo* himself to know what it was designed for;—not but that there are frequently such *Complications*, and *Contraindications* too, sometimes in diseases, as makes some degree of *Combination* and Contrast in a medicine necessary.”

The concluding advice, after recommending great care and exactness in regulating diet and regimen, deserves particular attention. “ As for those who will neither read nor reason, but practise by Rote and

\* In 1747.

prescribe at a Venture, I must seriously advise them at least to peruse the Sixth Commandment.”\*

Whether the Fellows of the Royal College applied to themselves this advice of one of their associates, we have no means of determining. All that can with certainty be said is, that individual physicians had made greater advances in the simplification and improvement of the Materia Medica and its practical applications, than the Royal College of Edinburgh or even London had done. It was therefore absolutely necessary for the College to make an effort to bring their Formulary up to the level of the knowledge and practice of the day ; and, accordingly, in the course of nine years after the publication of the fourth edition, namely, in May 1753, they began to prepare another edition, which appeared in 1756. In the preface, it is said, that though few new compositions are added, yet several are amended, and some which the superstition or credulous facility of their ancestors had introduced, were finally banished.

The improvement is indeed considerable, though the list of Vegetable Simples is still luxuriant ; and though among the Animal substances we find Cobweb, Snails, Earthworms, Millepeds, and Viper, there is no place for Human Skull, Mummy, Goose and Duck Fat, the Scorpion, or Excrement of the Dog.

In the Formulary, the *Confectio Damocratis* and

\* An Essay on Fevers, &c. The Third Edition. By John Huxham, M.D., Fellow of the Royal College of Physicians at Edinburgh, &c. London (1757. Preface, p. xiv).

the *Theriaca Andromachi* are banished. The *Emplastrum Defensivum* is reduced to six ingredients.

Among the list of members at this time are seen, besides the names formerly mentioned, those of William Cullen, Francis Home, and Alexander Monro. Robert Whytt was at this time in the University.

The Edinburgh College, indeed, by the publication of this edition, had taken a step in advance of the London College. They had rejected the Confection and the Theriac, which the London College had not yet done. They had also throughout studied greater simplicity in composition than the London College had done. In short, the Fifth Edition of 1756 was the first respectable Pharmacopœia which had yet been offered to the profession.

The College advanced steadily in the path on which they had entered. Much superfluous, redundant, and unprofitable matter had been cleared away. But much still remained ; and it was manifest that a clear and intelligent head, a mind unfettered by prejudice, and guided by decision and firmness as well as knowledge, were requisite, in order to complete such reforms as the state of medical science in general, and the interests of the public required. These qualifications were happily united in Dr Cullen, who, it has been already stated in a former part of this volume,\* had bestowed upon the composition and arrangement of the proposed new Pharmacopœia, for some years previously, a great deal of pains.

\* Page 81, 82, &c.

The work, as is also there stated, appeared in July 1774,\* during the period that Dr Cullen held the office of President. Of this edition, which was the sixth, and the changes by which it is distinguished from all its predecessors and contemporaries, a notice sufficiently detailed has been given in the same place, to render it unnecessary here to recur to the subject. It may be merely remarked, that not only is the long and multifarious list of the *Materia Medica* greatly curtailed by the ejection of many useless articles, especially from the Vegetable Kingdom ; but all the offensive and absurd articles of the Animal Kingdom, with the single exception of Millepeds, are discarded. Snails, Earthworms, and Viper, are no longer seen.

A most beneficial service it undoubtedly was to abridge the Catalogue of the *Materia Medica* ; for by retrenching not a few useless and superfluous articles, and selecting only those which experience had shown to be most suitable for the purposes of the physician, the whole was rendered more efficient. The *Pharmacopœia* of 1774, was in this respect a sort of product of the Lectures delivered by Cullen in 1761, while it was a precursor of the more finished work which we are forthwith to consider.

To any one who takes a view, at once comprehensive and just, of the influence of the various circumstances above noticed, it will not be difficult to perceive, that the experimental researches and reasonings of Haller, and the profound and acute re-

\* 19th of July is the date of Preface.

flections of Robert Whytt, were of great, perhaps incalculable service, in leading physicians to think more justly than previously, both upon the powers of the system in resisting and contending against disease, and on the mode in which remedial agents of every kind operated. On remedies for diseases, and medicines, Haller thought little; Whytt not a great deal. But the researches of the former on the Circulation, and on the Irritability of the various organs, and the observations and reasonings of the latter on the deranged conditions of the Nervous System, and the influence of that system on the induction, the maintenance, and the removal of disease, were calculated to open new and more just views of the powers of the animal body, and to lead physicians to consider by what means they could avail themselves of the operation of these powers in the cure of diseases. That they actually did so with a certain proportion of the profession seems certain.

It may be not unseasonable to add, that the Seventh Edition of the Edinburgh Pharmacopœia, with further emendations and corrections upon the sixth, was published in 1782–1783. The London College, which, after their great effort in 1744, had remained silent and inactive, now began to prepare a New Edition. This appeared in 1787; and presented unequivocal proofs of having adopted all the improvements of the Edinburgh Formulary.

From this unavoidable digression on the state of the British Pharmacopœias of the eighteenth century, I return to the works on *Materia Medica* immediately previous to the time of Dr Cullen. It is

unnecessary, however, to advert to the numerous Treatises published in the German States, in the Netherlands, in Italy, and in France. Some, and only a select few of these, have been noticed by Cullen in his preliminary sketch. Nor is it requisite to say much of the works of M. Lieutaud and M. Venel, after the character of these performances given by Cullen. The Treatise of M. Lieutaud, which appeared at Paris in 1781,\* is, notwithstanding the high reputation of the author, in almost all respects far below the level of the Pathological knowledge even of that time; and if it contain some few pieces of information not wholly erroneous, it abounds on the other hand in useless, obsolete, and almost childish matter. The concluding part of the work, on Aliments, is little more than a series of idle truisms, without any of those general views upon Dietetics, and the characters of alimentary articles, which it might have been justly expected to contain.

The work of M. Venel has the advantage of being more practical and less encumbered with irrelevant matter than the Treatise of M. Lieutaud.†

Two English works only are here entitled to be mentioned; The Experimental History of the Materia Medica, by Dr William Lewis, and the Lectures of Dr Charles Alston.

\* Précis de la Matière Médicale. Paris, 1781. Deux Tomes 8vo.

† Précis de la Matière Médicale Par M. Venel. Augmenté de Notes, &c. Par M. Carrere. Deux Tomes 8vo. Paris, 1787.

Dr Lewis, it has been already said, had, in 1759, introduced to the English reader the Chemical Works of Caspar Neumann ; and as these contained a considerable proportion of Pharmacological information, with occasional medical remarks, he had an opportunity of exercising his judgment in the duty of selection, recension, adaptation, and general amendment and correction. The work which Dr Lewis produced in 1761, and of which a second edition, with corrections and additions, appeared in 1768, was in all respects the best that had appeared up to that date. Though arranged in alphabetical order for facility of consultation, it contained, in a clear, succinct, and precise form, all the requisite and most trust-worthy information on the natural and chemical history of medicines, the most eligible modes of preparing them, and the medical uses to which they are applied. Dr Lewis gives no list or Catalogue of *Materia Medica*, so that it is not easy to say whether he abridged the lists or not. He was, however, cautious and reserved, both in admitting articles as medicines, and in assigning effects either physiological or therapeutic ; and though the work contains a few substances which a more fastidious inquirer would have rejected ; yet, on the whole, we meet with few articles calculated to excite distrust and awaken ridicule ; and when these are mentioned, they are subjected to criticism and censure. The Preface to this work is written in a sensible and philosophical spirit, and shows that the author had formed just conceptions of the nature and objects of a useful Treatise on *Materia Medica* ; and through-

out the whole work, Dr Lewis appears to have laboured diligently and judiciously to improve, amend, and utilize this department of Medicine.

The Experimental History was, after the death of Dr Lewis, published\* by Dr Aikin of Warrington, with additions and corrections, and in that shape it received the commendatory testimony of Dr Cullen.

The Lectures on Materia Medica by Dr Charles Alston were published in the year 1770, about nine years after the death of the author, by his successor in the Chair of Botany, Dr John Hope. Of this Treatise, it is remarked by Dr Cullen, that "it must be supposed to have been composed long before its publication." Cullen was evidently desirous to speak tenderly of the character and labours of an industrious teacher, who had been his colleague in the University. Not only are the Lectures open to the objection now stated, and which acquired increased force after the lapse of nine years ; but it is further manifest, that for many years previous to his decease, Dr Alston had done nothing to carry forward the knowledge of Materia Medica to the time at which he lived and lectured.

It must be allowed that the work is elaborate, evinces learning and knowledge, and may be read as a historical guide to show what Geoffroy and various preceding authors taught. The natural and commercial history of the articles is given in general with correctness, and as much chemical information is communicated as could be expected from the times. But in all the essential elements of a useful

\* In 1783 and in 1789.



work on *Materia Medica*, in Physiological and Pathological knowledge, in just Therapeutic principles, in sound judgment, and caution in forming an estimate of the efficacy of remedies, there is an amount of deficiency which it is mortifying to behold. Dr Alston had formed very enlarged, if not exaggerated and extravagant, notions of the importance of *Materia Medica*, and the time and attention required to study it properly ; and like all persons of this class, he seems to have thought that in the treatment of diseases all was to be effected by Medicines, and nothing by Diet, by suitable exercise, or the powers of the System ; and that every statement reported of Medicines and their effects was to be implicitly believed. In this spirit he has collected a large amount of information from various sources ; in some instances not very judiciously. The work is prolix and tedious ; and had it been reduced to one-half, or even to one-third it would have been more useful, and left the reputation of the author in a better position. It was at all events clear, that the work was unfit to serve as a guide to the medical practitioner in acquiring a serviceable knowledge of *Materia Medica*.\*

\* It may be proper to remark, that Dr Alston was Professor of Botany, and taught *Materia Medica* only as a branch of Botany. Without any intention to undervalue botanical knowledge in general, it may be observed, that the association of lectures on *Materia Medica* with Botany is not the plan which is calculated to render them most profitable to the student of medicine.

The knowledge of Botany and Natural History, however important it may be in reference to procuring and distinguishing medicines or the substances from which they are obtained, can

The evils to which attention has been hitherto directed, were in general the consequences of indolence, ignorance, undue and thoughtless veneration for antiquity and previous authority, in some instances of superstition and love of the marvellous. But soon after the commencement of the eighteenth century a new source of fallacy and perversion came into operation; and the influence of this was, if possible, more powerful, and less easily counteracted, because it assumed, and actually bore the aspect of scientific inquiry and experimental investigation. Near the close of the seventeenth century, namely, in 1679, John James Wepfer, a zealous and well-informed Swiss physician,\* published the results of an Experimental Inquiry into the Poisonous Effects of the Water Hemlock (*Cicuta Virosa*); and, as illustrative of these, of the effects of *Conium Maculatum*, Aconite, *Cocculus Indicus*, Nux Vomica, Hellebore, Jalap, *Fritillaria Imperialis*, *Solanum Dulcamara*, *Atropa Belladonna*, Henbane (*Hyoscyamus*), Bitter Almonds, and, in a cursory manner, of the effects of Antimony, Arsenic, and Mercury. The results of this inquiry seem not to have attracted much attention till the year 1715; but after this time they be-  
 avail but little in explaining the action of remedies upon the human body.

The therapeutic method, founded upon the study of the nature and effects of disease and the action of remedies, can alone form a sure foundation for sound practical knowledge in this department.

\* Wepfer was a native of Schaffhausen, Archiater or Principal Physician to the Town of Wurtemberg. Born in 1625, died in 1695.

came more fully known in France, Germany, and the Low Countries ; and they naturally created, and increased where it already existed, the taste for ascertaining experimentally the effects of various articles employed as medicines, but which were known to possess poisonous properties. In another mode, the researches of Wepfer were useful. The number of animals which were destroyed in his experiments was considerable ; and he not only gives correct accounts of the anatomical structure of these animals, some of which, as the eagle and wolf, had not been previously examined ; but he records various important physiological facts which had fallen under his observation in the course of these experiments.\*

During the close of the seventeenth century and the first half of the eighteenth, *Ipecacuanha* became an object of great interest, and was the subject of many observations. During the same time, and somewhat previously, *Arnica montana*, or Leopard's Bane, attracted the attention of various physicians in Germany, and continued to hold its position as an agent much used in certain affections of the locomotive organs till far on in the eighteenth century. Valerian was esteemed by not a few as a serviceable remedy in certain affections of the Nervous System.

\* *Johannis Jacobi Wepferi Historia Cicutæ Aquaticæ ; Quæ non solum Plantæ hujus Venenatæ Structura Naturalis, Vires, et Operationes deleteriæ in Hominibus ac Brutis accurate describuntur, sciteque explicantur ; sed et aliorum plurimorum Venenorum Qualitates funestæ deteguntur. Curante Theodoro Zuingero Archiatro Basil. Cum figuris æneis. Lugduni. Batav., 1733.*

Lastly, Seneka, or Rattlesnake Root (*Polygala Senega*), was, in 1738, strongly recommended by John Tennent as a powerful remedy in certain forms of low Inflammation of the Lungs.\*

In 1743, appeared a new candidate for popular favour. George Berkeley, Bishop of Cloyne,† brought forward the hygienic properties and therapeutic powers of Tar-Water in many different, sometimes opposite, morbid conditions ; and, in the course of one or two years, his recommendations were zealously supported by friends, and confuted and resisted by opponents. In this instance, the rank, the learning, and the high character of the author for piety, integrity, and benevolence, accomplished, for a considerable time, that which should have been left to be effected by observation and experimental trial. Notwithstanding opposition of every kind, Tar-water was much and extensively employed both in England and in Ireland. In several of the Tracts written against the medicinal uses of Tar-Water, there entered probably too large an amount of acrimony.‡

\* An Epistle to Richard Mead, concerning the Epidemical Diseases of Virginia, particularly on Pleurisy and Peripneumony. Wherein is shown the surprising Efficacy of the *Seneca Rattle-Snake Root* in Diseases owing to a Viscidity and Coagulation of the Blood, &c., &c. To which is prefixed a Cut of the Plant, and an Appendix annexed. By John Tennent. Edinburgh, 1738. 8vo. Pp. 102.

† *SIRIS* : A Chain of Philosophical Reflexions and Inquiries concerning the Virtues of Tar-Water, &c. By the Right Rev. Dr George Berkeley, Lord Bishop of Cloyne. A New Edition. with Additions and Emendations. Dublin ; reprinted, London, 1747. 8vo.

‡ An Authentic Narrative of the Success of Tar-Water, &c.

But it is impossible to deny, that the remedy, if to that character it were entitled, was praised far beyond its just merits, and that to its use were ascribed effects which it was perfectly clear were not produced by it.

Soon afterwards, namely, in 1746, appeared the experiments of Browne Langrish\* on the Effects of Cherry Laurel Water on various animals. From these experiments, which were in a great degree suggested by the facts recorded by Dr Madden, and the experiments performed by that physician and Dr Cromwell Mortimer, Langrish thought there was reason to infer that the Cherry Laurel might be beneficial in the cure of diseases.

The taste thus created for new remedies increased as the eighteenth century advanced; and after the middle of the century, the activity, especially of several German physicians, was incessant in trying and recommending for the cure of various painful, and generally incurable maladies, the employment

By Thomas Prior, Esq. A New Edition, Complete. Dublin and London, 1746.

A Cure for the Epidemical Madness of Drinking Tar-Water. By T. R., M.D. London, 1744. (Tancred Robinson?) June 1st.

*Nosologia Empirica Examinata*, or a Specimen of Partiality and False Reasoning. By a Physician. London, 1747. Pp. 46.

Remarks on the Bishop of Cloyne's two Letters, &c. Anonymous. 8vo. Pp. 48.

\* Physical Experiments upon Brutes, &c., to which is added a Course of Experiments with the Lauro-Cerasus. By Browne Langrish, of the College of Physicians, London, and F.R.S. London, 1746.

of remedies either entirely new, or previously little known, or known chiefly as poisons of various degrees of intensity. In this manner, hemlock was recommended by Antony de Stoerck,\* John Henry Rahn, Locher, and John Fothergill, in Skirrhous Tumours, Cancer, Tumours which must have been Strumous, probably Irritable or Neuralgic; in Caries, and bad forms of Syphilis, and Mercurial diseases. Deadly Nightshade (*Atropa Belladonna*), was recommended by Münch as a remedy against Canine Madness,† and by others as a remedy against Skirrhous and Cancer. Thorn-Apple (*Datura Stramonium*), was recommended as an efficient remedy in certain forms of Cerebral Disorder by Antony de Stoerck, who became, indeed, between 1760 and 1771, the unwearied advocate of the therapeutic powers of various active Plants;—Aconite, Hyoscyamus, *Clematis*, *Dictamnus albus*, *Pulsatilla* or *Anemone*, Meadow Saffron.‡ The characters assigned to these agents, some powerful, others inert, were not in all instances confirmed by subsequent observation; and it was afterwards said, that De Stoerck, who was then at the head of the Medical Department of the Austrian Army, often lent too easy cre-

\* Antonii de Stoerck Annus Medicus. Vindob. 1760–1761.

† D. F. Münch de Belladonna Efficaci in Rabie Canina. Ext. Frank Delect. i. p. 248.

‡ Antonii de Stoerck, Experimenta et Observationes circa Nova sua Medicamenta. Vindob. 1765.

*Id.* De Flammula Jovis sive Clematide Erecta, et de Dictamno Albo. Vindob. 1769.

*Id.* De Pulsatilla Nigricante. Vindob. 1771.

ference to the reports of young physicians who found that testimonies on the medicinal properties of new or little known plants formed a sure means of obtaining from him offices of trust and employment in the Austrian Army.

Meanwhile, the work of experiments and observation ceased not to proceed. The powerful effects of Foxglove were made known by Darwin, Withering, and Ferriar. The search for substitutes for Peruvian Bark, when that article was scarce and costly, led to trials with various indigenous and more accessible barks; and among these the labours of different inquirers, in process of time, furnished the Crack Willow, the Horse Chestnut, the Quassias, Colombo Root, Winter's Bark, Oak Bark, and similar substances. For other purposes parts of other plants were more or less strongly recommended, as Mezerion, Rhododendron, Spigelia, and *Dolichos pruriens*. Of these, however, it is unnecessary to say more.

The consequence of all this mental activity in the search for medicines was, that many were recommended in the strongest terms as unfailing remedies for the cure of various painful disorders and unmanageable maladies; and by a large proportion of the profession, all that was stated at the time was partly or implicitly believed. Nothing is more certain, nevertheless, that in very few, or not more than one or two instances, have the representations made of these articles been realized. Observers and experimentalists did not distinguish between physiological effects and therapeutic efficacy. Physiological effects

were in general sufficiently manifest. But therapeutic and curative results were rare indeed. Of all the substances, during the period referred to, subjected to trial, Ipecacuanha and Foxglove only have kept their positions as therapeutic agents. Whether Cherry Laurel Water, now represented by Hydrocyanic Acid, has been a useful acquisition in the hands of the physician, I leave to others to say.

In the foregoing sketch, I have not attempted to give a history of *Materia Medica*, and its progress and vicissitudes. The duty has been performed by Dr Cullen with learning and judgment; and in his preliminary Discourse readers will find a correct and instructive view of the History of *Materia Medica*, and the principal authors by whom its study has been cultivated. My purpose has been in some respects different, and has led me to direct attention to circumstances which Dr Cullen thought it superfluous to notice, or which the plan of his Discourse did not require him to consider. My endeavour has been to present, not a complete delineation of the state of *Materia Medica* previous to and at the time of Cullen, but only such a view as might enable readers to form their own opinions on this Department of Medical Science between 1780 and 1785; to explain how little real benefit and improvement had been effected, notwithstanding the efforts of many labourers; and to show how much was yet required to be done, in order to place *Materia Medica* in a just, respectable, and serviceable position.

The facts adduced clearly show, I think, that *Materia Medica* was not only in a backward state in



relation to other branches of Medical Science, but that, notwithstanding the efforts of a number of industrious and well-informed physicians, persons who had at once sagacity to recognise its deficiencies and its failings, its superfluities and misapplications, and judgment to show how these were to be rectified; yet from various causes the Department continued in a condition behind that of other branches of medical knowledge; exuberant and redundant where it ought to have been retrenched, loaded with inert and antiquated articles, and unscientific and useless compositions, and in short the general receptacle of all the follies, superstitions, and extravagancies of the medical profession. There were no general principles to guide the mind of the physician. It seemed as if Physiology and Pathology had been studied in vain; for either no application of their doctrines was made to Therapeutics and Materia Medica, or if such application was made, it was in a manner so illogical, so inconsequent, so unreasoning, that no other inference can be deduced, than that the moment the physician turned to his Pharmacopœia and Dispensatory, he forgot or disregarded scientific principles, and was in danger of being carried away by blind empiricism.

Amidst the state of matters here delineated, and in the face of the difficulties and obstacles above mentioned, it was, that Dr Cullen produced his work on Materia Medica. It comes now to be shortly considered, to what extent that work was calculated to remove the evils which were then prevalent, to supply deficiencies long felt, to eradicate prejudices, to

correct fallacies, and to serve as a scientific and rational guide to medical practitioners.

The treatise on *Materia Medica* by Dr Cullen may be regarded as consisting of Three General Divisions ; the first commencing with the Historical Discourse, and containing an introduction, in which are explained certain primary principles, Biological and Pathological ; the second treating of Alimentary Articles, or substances employed as food, drink, and seasonings ; and the third treating of those substances which are employed as Medicines, either in preserving health or in curing disease.

Of the Historical Introductory Discourse I have already spoken, as giving a learned and judicious view of the subject down to the time at which Dr Cullen wrote.

In the Physiological Introduction which follows the History, the object of the author is to consider and explain those general principles which relate to the action of all sorts of food, drink, and medicines, upon the human body ; in other words, to consider those properties and powers of the human body as an organic system, which are called into action by the agency of food, drink, and medicines. To consider those general principles, he remarks, is the more requisite, because several physicians appear not have bestowed on them the attention to which they are entitled ; and because, further, upon the justness and propriety of these principles physicians were by no means agreed. On some of these principles, his own opinions, he allows, are peculiar ; and independently of this, he believed it to be his duty

to explain certain new principles, the existence and operation of which he found it requisite to admit. Dr Cullen here refers to the doctrines which he taught on the conditions and powers of the Nervous System, the laws by which those powers are regulated, and the influence which this system exerts over the organs and functions of the entire frame.

Assuming as either self-evident or sufficiently clear not to require demonstration, the principle,—that medicines and all substances act upon the living body in a manner totally different from that in which they act on inorganic matter or on the dead body, he proceeds to explain the peculiar circumstances of the animal body, by means of which it is thus capable of being acted upon in a peculiar manner by other bodies applied to it or introduced within it. These circumstances are the possession of a Nervous System, and the properties of Sensibility and Irritability.

This Introduction he then divides into three chapters; the first devoted to the Action of Medicines on the Body in General; the second, On the various means of our learning the Virtues of Medicines; and the third, On the most proper plan for a Treatise on *Materia Medica*.

From the fact of the possession of Sensibility and Irritability, and the first of these properties being resident in all those textures to which any portion of the Nervous System is distributed, Dr Cullen makes the general inference, that the peculiar effects of substances in general, and of those substances in particular which are called Medicines, when applied to the human body, depend upon their action upon

its Sentient and Irritable parts;\* in other words, that the peculiar action of medicines universally depends upon motions excited and propagated in the Nervous System. "These, therefore," Dr Cullen adds, "are the conditions of the Living Body which we were engaged to explain. What is the nature of the matter in which these motions take place, or in what manner it is inherent in the Nervous System, is not well known. But we think it may be justly held as existing, and may speak of it under the appellation of the *Nervous Power*. As it appears only in the living body, and disappears entirely in the dead state of the body, it may be otherwise properly enough termed the *Vital Principle*."†

I am rarely to enter into commentary or criticism upon the doctrines maintained by Dr Cullen; as the nature of this work, and the limits within which it must be confined, admit not easily of such disquisition. But I may here remark, that while on the one hand the above statement contains an abridged view of much that is peculiar to Cullen in his reasonings on the action of Alimentary and Medicinal substances, the exposition here given of the action of these substances is more exclusive than can be warranted by subsequent researches. The Nervous System, it must be admitted, is the great general medium by means of which all agents act upon the animal body. But the experiments of various enquirers during the last thirty or forty years, have

\* P. 58. Treatise of the Materia Medica. Introduction. Chapter i. 4to. Edin. 1789.

† Materia Medica, pp. 58, 59.

led Physiologists to modify in some degree their opinions both regarding the primary and the ultimate operations of medicines, of alimentary articles, and of poisons. The researches of some of these inquirers, indeed, in directing attention to the channel of introduction, have tended to modify the doctrines of Physiology as to the subsequent action. These researches show, that a very general channel for the action of food, medicines, cordials, and poisons, is the absorbing faculty of the venous system ; and it is to be remembered, that this absorbing operation proceeds not only in the interior of the stomach and alimentary canal, but also upon the cutaneous surface, and upon the surface of the lungs, and wherever there is a surface exposed by a wound.

Further, it must be remarked that the phenomena which, between 1826 and 1828, M. Dutrochet described under the names of *Endosmosis* and *Exosmosis*, and the researches of Graham and others on the tendency to mutual diffusion among gases, have placed all the inquiries relating to this subject in positions very different from those in which they stood in the time of Dr Cullen. This subject, indeed, has suggested so many new questions, that their most brief examination would lead to discussions much too lengthened for any special notice in these pages.

In his concluding paragraph, Dr Cullen makes, regarding the identity of the Nervous Power and the Vital Principle, a statement, the validity of which has been often called in question, and which would

assuredly be, if not opposed, at least not received by many of the Biologists of the present day. It would be quite out of place here to enter upon the question of the existence or nature of what is the Vital Principle, upon which volumes have been written, and much controversy has taken place, very unprofitably indeed. Nor is this necessary ; as the question of the existence of a Vital Principle as a means of explaining phenomena in the animal body has been considered at sufficient length in the First Volume of this work, by Dr Thomson, when speaking of the doctrines taught by M. Barthez of Montpellier.\*

The discordance of sentiments, indeed, evinced by all those who have written on this subject, and the complete absence of useful results, have led the majority of physiological writers to regard the inquiry as either unprofitable or beyond the reach of the human faculties. I hasten, therefore, to quit the subject, and merely observe, that though it appears to be a mistake to identify the Nervous Power with the Vital Principle, yet this assumption was sufficiently accurate for the purpose of Dr Cullen, in explaining the action of Medicines upon the Human Body ; for it is next to certain, that without the agency of the Nervous System, neither food nor Medicines can act ; and it is quite certain, that they do not act, unless upon bodies still possessed of Life and Vital Action.

In the First Chapter, which is subdivided into three Sections, Dr Cullen, with the purpose of ex-

\* Life of Cullen, Vol. I., p. 443, &c.

plaining what ought to be understood of Temperaments, gives a short view of the leading characters of the Simple Solids, of the State and Distribution of the Fluids, and the State of the Nervous Power as to Sensibility and Irritability; as to Strength and Weakness; and then treats of particular Temperaments,—the Sanguine, Melancholic, Choleric, and Phlegmatic; and of Idiosyncrasies.

It cannot be necessary in this place to enter into any detailed account of the mode in which Cullen states the then known facts, and explains his views on these topics; for the reason that the whole of the facts, and all the most important reasonings of the author, have been already given in the Institutions of Medicine, either by Cullen himself, or by Dr Thomson in the republication of that Treatise in his Edition of the Works of Dr Cullen in 1827. So much, indeed, of this Introduction has been transferred to that short but clear summary of the Principles of Physiology; so complete has it thus been rendered, that all that I conceive to be required here is, that I make a general reference to these Outlines.\*

In the Second Chapter of the Introduction, Dr Cullen considers, in four separate articles, the fol-

\* The Works of William Cullen, M.D. &c., Edited by John Thomson, M.D., F.R.S.L. & E. In Two Volumes. Volume I. Edinburgh and London, 1827. The passages transferred from the *Materia Medica* to the *Institutions of Medicine* are all marked by the letters M. M. The reader may refer in this instance to page 192, Chapter II., pages 197–200, p. 205, 206. Then of Temperaments, pages 214–223.

lowing questions : The use of Chemical Analysis in investigating the Virtues of different Substances ; the use of Botanical Affinities in ascertaining the Medical Virtues of Plants ; the Sensible qualities of Substances as indicating the presence of Medicinal Properties ; and on acquiring knowledge of the Virtues of Medicines by experience. Upon the three first heads the observations are short, but perfectly appropriate. On the last head, which is by far the most important, and which, in the time of Cullen, had been assuming a position of great and imposing influence, Dr Cullen takes a careful and philosophical survey of the various mistakes and fallacies which beset the Inquirer in this course of investigation, and to which, indeed, may justly be ascribed a large proportion of the absurd and silly medicines and medicinal compositions, with which at different periods the therapeutic part of medicine has been encumbered and degraded.

In the Third Chapter of the Introduction, Dr Cullen shortly considers the most proper plan for a Treatise on Materia Medica ; and after referring to those of Boerhaave, Linnæus, Bergius ; the method according to sensible qualities by Cartheuser and Gleditsch ; the alphabetical method of Neumann and Lewis ; the mixed plan of Alston and Vogel ; he adopts that according to the Medicinal Virtues or the Physiological and Therapeutic Properties.

In adopting this plan, he was guided by the natural desire to administer remedies according to general indications ; that is, according to what may be inferred, after forming a just idea of the nature



of the disease, to be the objects or purposes to be kept in view by the physician with the intention of curing it, and preventing it from producing hurtful effects upon the organism.

“ As it seems proper,” he observes, “ that every practitioner ought, as far as possible, to practise upon general indications ; so it is evident that his study of the *Materia Medica* is especially to know the several means that can answer these. Such a plan must be the most proper for giving a student instruction ; and if, while medicines are arranged according as they answer general indications, the particulars be likewise thrown together, as far as possible, according to their sensible qualities and botanical affinities, this plan will have the advantage of any other that has been proposed for presenting together the subjects that ought to be considered at one and the same time, and give the best means of recollecting everything that relates to them.”

He finally adds, that he is “ particularly willing that this *Treatise of the Materia Medica* should be considered as giving a *Therapeutice* or *Methodus Medendi*, from which part of the medical system the *Materia Medica* cannot properly be separated. It may indeed be alleged, that as the *Therapeutice* must be founded on a particular system of Physiology and Pathology, so it must be liable to all the errors and fallacies of these : but every treatise on the *Materia Medica* which refers the virtues of medicines to general indications must be exposed to the same objections ; and though we cannot presume to say that our plan in this respect shall be without mistakes, yet our general plan in most of its parts being nearly the same with most other systems, we trust it shall not be very

faulty : And as it is a principal purpose of this Treatise to render the *Methodus Medendi*, or the establishing of general indications, more correct, and better adapted to the particulars of the Materia Medica than it has hitherto been, so it affords a particular reason for our following this plan, which in general is very much the same with that of Dr BOERHAAVE in his Treatise *De Viribus Medicamentorum*, and such as has been followed by several late authors, as SPIELMAN, LOESECKE, and LIEUTAUD."

The Chapter is concluded with a Glossary or Dictionary of General Terms employed by writers on Materia Medica ; a General Table of the Articles of the Materia Medica, arranged, as nearly as possible, according to their effects upon the Animal Body, and a Catalogue of all the Articles employed. The latter may be regarded as a Synopsis of the Work itself. It consists of Two Parts : the First, on Alimentary Articles ; the Second, on Substances employed as Medicines.

In any Treatise on Alimentary Articles which is intended to be of true service to the physician, it is necessary to introduce as correct a view as possible of the general principles of Dietetics. It becomes requisite to consider not only the digestive susceptibilities and the nutritious properties of articles of food, not only those qualities in the substances used, on which depends their suitableness to serve as nutriment, but also those properties and actions of the alimentary canal, by the agency of which food is made more easily to undergo those changes, by means of which the growth and the vigour of the

system is maintained ; in short, by means of which the system at large is nourished.

Of this purpose, Dr Cullen had formed a just idea in composing the first part of his work ; and, indeed, he is almost the only writer who at that period had formed ideas at once just and accurate on this subject.

The proposition of referring all necessary alimentary articles to the three heads of Acid, Sugar, and Oil, is an attempt at analysis and generalization which, though laudable and ingenious, will scarcely bear strict criticism.

Under the head of Acid, Dr Cullen included not only those acids, as the citric, malic, and tartaric, which are found in fruits ripening and ripened, but that acid which is the result of fermentation in vegetable bodies ; vinegar, and its modifications. Now, though it must be admitted that the former, or what are termed native vegetable acids, are employed by the human race, they form too small a proportion of alimentary matters, properly so named, to be received as principal and important members of the alimentary class. It is only at certain seasons of the year, that they can be obtained in perfection and abundance ; and though they are much employed in the southern and tropical regions, it can scarcely be said that they are necessary alimentary articles. It is chiefly under certain circumstances of disease, scurvy especially, and where an acidifying diet is supposed to be beneficial, that the vegetable acids are employed as articles not of sole, but partial nutriment. It has been further ascertained, that most

of these acids are most conveniently used when conjoined, which, in nature, they generally are, with potass, when it has been found that they are, after being taken into the animal body, decomposed and converted into carbonic acid, forming in this manner carbonate of potassa. All these vegetable acids, indeed, consist of Carbon, Hydrogen, and Oxygen, combined in such a manner that the reducing power of the alimentary organs is sufficient to effect their disunion, and consequent conversion into carbonic acid.

Vinegar is perhaps still less entitled to be ranked as an alimentary article. But this Cullen perceived; and he limits his character to the native vegetable acids. Its proper position appears to be among condiments, and, as such, to be employed both sparingly and with caution.

Under the head of Sugar Dr Cullen ranked not only the sugar of Sugar-Cane, the Maple, the Palm, and the saccharine fruits, as figs, dates, and similar products, and the saccharine roots, as beet, carrot, turnip, parsnip, but the Amylaceous matter of the Cereal grains, for the reason that, upon being made to germinate, they furnish malt. It cannot be said that this is an error, or even a forced generalization. Its principal objection is, that it withdraws the mind of the reader from the most important to the least important alimentary property of the substance under consideration. It is not in its pure form that sugar can be justly regarded as alimentary, but in that mixed and diluted state in which nature furnishes it in the saccharine fruits and in the saccharine or

saccharo-amylaceous roots and tubers. Pure sugar has been long known to be not endurable by the human stomach ; and experiments have shown that it is not only not fit to sustain life and nourish, but that it is actually hurtful, and, if continued long, may cause death.

With Starch, the *Fecula* of former chemists, Amidine or Amylaceous matter, the case is entirely different. That substance forms the largest proportion of all the Cereal grains employed as food, and, with variable proportions of Vegetable Albumen or Glutine, is the great basis of the nutriment derived from these substances. It is, indeed, the presence of this Amylaceous matter with Albumen that gives to these grains, and the flour made from them, the farinaceous character, to which Dr Cullen himself attaches great importance as nutritious elements (p. 231), and which renders them fit to be converted into bread. It appears, therefore, that a preferable mode of arrangement would have been to assign to Starch or Amylaceous matter the principal place ; and to this to refer Sugar and the Saccharine articles as subordinate nutritious principles.

It may tend to show the advantage of this method of arranging Starch as an alimentary principle, that it would have thus readily received such substances as Gum, Salep, Tapioca, and similar articles, which Dr Cullen, at a subsequent part of his discussion, refers to the head of saccharine aliments.

On the other hand, it must not be forgotten that Dr Prout, in his classification of aliments, has made his second principal head the Saccharine ; and, distinguish-

ing this into two orders, the crystallizable saccharine bodies, and the uncrystallizable or organized bodies, places sugar and vinegar under the former, and starch, lignin, and the different forms of gum, under the latter head. The principal advantage of this method is, that it establishes a broad and well-marked line of distinction between those saccharine aliments which should be taken only sparingly and along with others, and those which, from their fitness for nutriment, are taken alone, and can be used in greater abundance.

Dr Cullen, in considering the nutritious properties of the Cereal grains, and the flour thence produced, admitted the existence not only of this Sacchariferous Starch, but another ingredient, which he says is probably an oil of that mild nature "that is obtained from many farinaceous seeds by expression," and is therefore commonly named by the general title of an Expressed Oil. This leads him therefore to consider what he supposes to be the other part of the vegetable aliment.

He assigns various reasons for believing that this oleaginous or fatty matter enters into the system of man and animals, both as forming part of the Farinaceous matter and separately. It is unnecessary, however, to reproduce his reasoning, which, on the whole, must be allowed to be well founded. His positions are in general sufficiently manifest; and if he has not assigned to the convertibility of starchy aliments into fat the amount of influence to which, in this inquiry, they are entitled, that must be ascribed to the imperfect state of animal chemistry at

that time, and to the little knowledge then possessed of the chemical relations subsisting between Amylaceous and Oleaginous alimentary substances.

In the observations now made on these relations, I remark, once for all, that it is incompatible with the nature of the present work, to recognise all the minute distinctions, which modern chemistry has found it proper to make of the different forms, under which Amylaceous matter appears; for instance, Amidin, Amylin, Dextrine, Hordein, and the various substances obtained from Amylaceous roots and stems. For the purpose of Physiological reasoning it is sufficient that the terms Starch and Amylaceous matter be understood to comprehend all those substances, which bear a general resemblance in physical properties, and contain a sufficient proportion of that principle which may serve as food.

Though the distinguishing properties of Gluten, as a peculiar principle in wheat flour, were made known to the Academy of Bologna by Beccaria in 1728, and published in 1745,\* and subsequently considered

\* De Bononiensi Scientiarum et Artium Instituto atque Academia Commentarii. Tomi Secundi, Pars Prima. Bononiæ 1745. 4to. P. 122.

Beccaria did not find Gluten in bean-meal nor in barley-meal, and says the experiment does not succeed in other grains, except in Maize.

Some discordance is perceptible regarding the exact time at which Beccaria recognised in wheat flour the existence of the Nitrogenous substance called *Gluten*. Dr Thomas Thomson places it in 1742; other authors in 1723. The Memoir containing the account of the experiments was communicated to the Academy of Bologna in 1728; and the editor of the Com-

by Kieselmayer, Dr Cullen seems not very willing to concede to it that influence as a nutritious principle which has since been admitted. "The discovery of Beccaria," he remarks, "can amount to this only, that besides the parts which he has assigned, there may be in certain vegetables a substance that makes part of the aliment which they afford ; and justly, indeed, as this newly discovered matter in its nature approaches more nearly to the nature of animal substance, than any other part of vegetable matter we know of. But with all this, we cannot find that this discovery invalidates our opinion of the chief part of the aliment afforded by vegetables being afforded by Acid, Sugar, and Oil, to be compounded by the powers of the animal economy."\*

It is somewhat singular that Dr Cullen did not perceive that, whether the fact of the discovery of Gluten as a peculiar principle did or did not invalidate his opinion, the admission of the existence of this substance in wheat flour, and even in smaller proportions in other grains and seeds, approaching, as he admits, more nearly to the nature of animal substance, would have rendered much more easy the explanation of the nutritious properties of vegetable substances, and rendered more intelligible the facility with which they are assimilated and transformed into the substance of animal bodies. Since the period at which Cullen wrote, the researches of Taddei on Gluten, and the experiments of Prout, Boussingault, mentaries published an abstract of this in 1745, in the second volume of the Commentaries.

\* *Materia Medica*. Volume I. Part I. Chapter I., p. 296.



and other chemists, by directing attention to the albuminous character of this principle, and the proportion in which it is found in different grains and seeds,—in short, to its wide diffusion in vegetable products, particularly in the Cereal Grains and Leguminous seeds,—have enabled physiologists to understand more clearly by what means vegetables nourish and sustain the strength of animals, supply materials for chyle and blood, and, indeed, by what means they maintain the vigour and repair the waste of the fibrinous textures. When it is known that good wheat contains about fourteen per cent. of this principle, the inferior sorts at least seven, and the medium qualities about ten or twelve per cent., and that Gluten itself contains rather more than fourteen per cent. of Azote or Nitrogen, or at least one-eighth part,\* it cannot be difficult to understand the great utility of this principle as a means of sustaining the strength and vigour of the animal body. The great importance of the presence of this principle is accordingly admitted by all the most trustworthy modern authorities,—by Prout, Thomson, Turner, Liebig, Gregory, Boussingault, Dumas, and Mulder. Candour, therefore, obliges us to admit that this is the most feeble and defective point in the doctrines of Dr Cullen. The mistake I have not attempted to evade, or even to extenuate. But it is surely just, while we show those defects which modern chemistry has enabled us to discover, to add, in the way of ex-

\* Chemistry of Organic Bodies. Vegetables. By Thomas Thomson, M.D., &c. London, 1838.

planation or excuse, that chemical knowledge was at that time in a very rudimentary condition; that the attempt made by Cullen was the first; and that till the time of Prout, in 1827, a space of nearly forty years, it cannot be said that any one attempted to follow him in the same path.

It was on the 14th of June 1827 that Dr Prout communicated to the Royal Society his Memoir on the Ultimate Composition of Simple Alimentary Substances; with some Preliminary Remarks on the Analysis of Organized Bodies in General. In this Essay Dr Prout distinguishes the principal Alimentary Matters employed by Man and the MAMMALIA into three great classes, namely, the SACCHARINE, the OILY, and the ALBUMINOUS. Under the first head Dr Prout comprehends not merely the different forms of Sugar usually included under that term, but all those vegetable substances, whatever be their sensible properties, into the composition of which Hydrogen and Oxygen enter, in the same proportions in which they form water. As the substances thus defined may generally be employed as aliments, and as they are derived from the vegetable kingdom, they may be considered as representing vegetable aliments; and in this view Dr Prout regards Saccharine and Vegetable aliments as synonymous. The Albuminous aliments contain Gelatine, Albumen, Fibrin, Casein, and Gluten, or Vegetable Albumen.\*

This method of division was more practical and

\* Philosophical Transactions, 1827, p. 355. On Stomach and Renal Diseases. Book III., Sect. I.

just. But it is liable to the objections already stated in regard to the predominant position assigned to Sugar and its modifications. For this and other reasons Dr Pereira proposed in 1843 to refer all alimentary principles to the twelve following orders.

- |                            |                    |
|----------------------------|--------------------|
| 1. Aqueous.                | 7. Acidulous.      |
| 2. Mucilaginous and Gummy. | 8. Alcoholic.      |
| 3. Saccharine.             | 9. Oily or Fatty.  |
| 4. Amylaceous.             | 10. Proteinaceous. |
| 5. Ligneous.               | 11. Gelatinous.    |
| 6. Pectinaceous.*          | 12. Saline.†       |

This classification, though more comprehensive, is probably something too minute. But this objection is of smaller force, than if, by being less minute, any important article had been excluded or placed in an inferior position. It appears to be manifest that, whatever arrangement be adopted, a prominent and independent position ought to be assigned to Amylaceous and Albuminous or Glutinous alimentary articles.

After a short passing notice of Gluten as defined by Beccaria, Dr Cullen adverts to the effect of the solvent juice or liquor of the stomach. But this relates chiefly to its action on different alimentary substances,—a subject on which he says very little. It is unnecessary, therefore, to examine this part of his discussion; and I merely observe that this division of the subject has been much more fully investigated,

\* Pectin; (*πησσω* and *πηγνυμι*;) Vegetable Jelly.

† A Treatise on Food and Diet, &c. By Jonathan Pereira, M.D., F.R.S., et L.S., &c. London, 1843. Chapter II., p. 87.

first by Dr William Stark, then by Sir Astley Cooper,\* and afterwards more extensively by Dr Beaumont of the Army of the United States.†

Dr Cullen then enters, in chapter second, on the consideration of Particular Aliments, and delivers the Medical History of these Aliments in two Sections;—the first on those derived from the Vegetable Kingdom; the second on those obtained from the Animal Kingdom. At the end of the chapter Dr Cullen, in a short appendix, gives his views on the art of Cooking.

In the two last chapters, (third and fourth;) Dr Cullen treats of Drinks and Seasonings.

It is quite unnecessary to enter into any detailed consideration of the manner in which Dr Cullen has treated the subject of Aliments in this part of his work. To do so in a proper manner, would far exceed the limits within which any observations of this kind, in the present work, ought to be confined; and, indeed, the only just mode in which a correct idea can be formed of the manner in which Dr Cullen considers the various articles employed as food, is by studying the work itself. It is sufficient to say, that the history of the different articles is delivered in a succinct, methodical, and perspicuous manner, free

\* Treatise on the Nature and Cure of Gout and Gravel. By Charles Scudamore, M.D. Fourth Edition, revised and enlarged. London, 1823. See p. 632.

† Experiments and Observations on the Gastric Juice and the Physiology of Digestion. By William Beaumont, M.D., Surgeon to the United States Army. Reprinted from the Plattsburgh Edition, with Notes by Andrew Combe, M.D., &c. Edinburgh, 1838.

from all the multiplied errors and idle fancies with which this division of Medicine was encumbered and sullied previous to the time of Cullen. Unfounded prejudices in favour of, or against particular articles of food, are discarded or refuted; just and rational views, founded on observation and experiment, are delivered; and throughout all the divisions of the Treatise, the reader perceives sound sense and just reasoning associated with practical information, in presenting judicious and serviceable views on the subject of Diet, its influence in preserving the health of the human frame, and its power in restoring health when impaired.

Of the characters here assigned, no part of the Treatise is without affording unequivocal and decided proofs. But if it be necessary for this purpose to refer to any particular portion, that which relates to the alimentary and hygienic properties of the Cereal grains and the Leguminous seeds, and that in which Dr Cullen delivers the chemical and medical history of Milk, and explains its effects as a dietetic, hygienic, and therapeutic agent, may be adduced as examples of the clear and short, yet instructive elucidation of an important subject not free from difficulty. In speaking of the Cereal grains, and considering their respective nutritious properties in comparison with those of the Leguminous seeds, Cullen makes a close approach to the views afforded by modern chemistry. Though, as already remarked in the general observations, he had not assigned to the combination of Gluten and Starch that influence as nutritious principles which experience,

as well as science, has shown that they possess ; yet for this apparent oversight he makes ample compensation by the more extended explanation which he gives in this part of his Treatise (p. 283); and he in a certain degree anticipates the important discovery of Legumine as a nutritious element, when he refers to the personal testimony of those who employed the meal of the leguminous grains as an article of food, both salutary and powerfully corroborating. (P. 293.) In this manner, those defects which were not easily avoided in the general History of Aliments, are supplied and corrected when treating of particular alimentary substances. I may be here permitted to remark that, in forming a just judgment of the merits of the scientific and medical writings of Dr Cullen, it is indispensable not to take isolated passages and portions, but to consider them in connection and in mutual relation, as integrant parts of one great and general system.

It is manifest that all articles of food ought to be considered in two points of view ; one as means of mere Nutrition, the other as Hygienic and Therapeutic agents, or as means of preserving health and restoring it when impaired. Though Dr Cullen has nowhere expressly stated this two-fold view, it is perfectly clear that he well understood it ; and nowhere, probably, does this appear more clearly, than in the account which he gives of the constituents of Milk, and of its peculiar suitableness as a salutary article of food, its powers in preserving health, and its applicability as an agent for the cure of certain morbid conditions of the system. His exposition of

the hygienic and therapeutic properties of Milk is perfect for the time at which it was published, and may at the present time be perused with interest and advantage.

The most decided and satisfactory testimony, however, to the merits of this part of the Treatise of Dr Cullen was the effect which it exerted upon succeeding writers on Diet and Regimen. It was not immediately that these merits were everywhere fully recognised; but when they were once recognised, the clearness, the simplicity, the reasonableness of the observations and instructions, gradually and steadily secured the approbation of the most competent judges, and the imitation of numerous able followers. For a long series of years subsequently, in no work was the subject of Diet treated, either professedly or incidentally, in which the authority of Cullen was not adduced, in which his manner of treating the subject was not more or less closely followed, and his views more or less accurately reproduced. It is not without interest to trace this influence even during the first twenty or twenty-five years of the present century. It would be tedious and unprofitable to mention all the works in which this is visible; but, as examples, I may refer to the writings of Schwilguè, Alibert, and Barbier, in which it is not difficult even for cursory readers to perceive, how closely the method of Dr Cullen is followed, how much his mode of viewing dietetic articles is adopted, and how indissolubly most of his physiological and pathological principles and reasonings had become entwined with the medical

doctrines and practice of the nineteenth century. These writers were aware of the truth of the remark of M. Hallé. “ Dans cet ouvrage de M. Cullen, dont je suis loin d’adopter toutes les idées, la partie qui concerne les Alimens, renferme d’excellentes choses, et qui méritent d’être profondément méditées.”\* Of English writers, as Willich, it is unnecessary to speak, as they are too numerous to be even mentioned.

It must not be imagined, nevertheless, that the precepts which Dr Cullen inculcated, and the facts which he stated in this portion of his Treatise, were adopted with blind and indiscriminate assentation by subsequent authors. Something of this was certainly perceptible in several works; and it would have been wonderful if this had not taken place. But it is the peculiar merit of Cullen’s Treatise on Materia Medica, and especially that on Dietetic articles, that it set the example of a new and more useful mode of acquiring and communicating information, and taught physicians that it was by inquiry, by doubt, by interrogation, by experiment, that trustworthy information can be obtained, and well-founded views on the effects of food, of diet, and regimen can be acquired.

It is creditable to the judgment, as well as the originality, of Dr Richard Pearson† and Dr John

\* *Encyclopedie Methodique*, Art. *Alimens*.

† A Practical Synopsis of the Materia Alimentaria and Materia Medica. A New Edition, comprising the Latest Improvements in the London, Edinburgh, and Dublin Pharmacopœias. By Richard Pearson, M.D., Member of the Royal College of Physicians, London, &c. London, 1808. 8vo.



Murray,\* that while they adopted many of the general principles of Cullen, his example taught them, that the most eligible course which an author on medical subjects can adopt, is to think and inquire for himself, to shun slavish subjection to any authority, and to take for granted nothing which is not ascertained by repeated observation. Cullen had carefully shown how much evil and error had crept into the *Materia Medica* and *Alimentaria* by this extreme deference to preceding authorities, and by receiving as evidence, for the truth of statements, the oft-repeated assertions of previous writers. All this unprofitable imitation the example of Cullen tended decidedly to discourage and abolish. “*Decipit Exemplar vitiis imitabile.*” The spirit of his writings, as his mode of thinking, was truly skeptical; that is to say, inquiring rather than doubting; and in the examination and discussion of every subject, one of the first proceedings was to propose the question, How many of the facts stated and believed were worthy of credit, and how many were doubtful and totally unworthy of credit? and on what evidence did each fact, if doubtful, rest? By this method of separating the wheat from the chaff, though he proceeded slowly, he advanced with more certainty to his object; and the results which he obtained, though small in appearance, were more valuable, and more useful for all the purposes of practical medicine. It was thus, not by the exact amount of knowledge and

\* *Elements of Materia Medica and Pharmacy.* By John Murray, M.D., &c. Two Volumes 8vo. Edinburgh, 1804, 1810, 1816, and Fifth Edition, 1828.

information which the writings of Cullen conveyed, that he rendered useful service to the medical knowledge of his own time. It was in a much greater degree by the spirit of inquiry and research which, both by example and precept, he inculcated upon pupils and followers; and by teaching them that, in a science like medicine, the first and most important duty is to exert the power of thinking independently, and to avoid adopting the opinions of others, unless they are confirmed by a reasonable amount of satisfactory and unquestionable evidence. The precepts and instructions, in short, of Cullen, and the severe logical manner in which they were conveyed, indirectly exercised a species of prospective operation, which may be observed at the present time; and if, in the subsequent writings of physicians, there be perceived on this subject more reason and less credulousness,—more common sense and less of fancy and caprice,—it is in a great measure to the influence of the work of Cullen, operating both by itself and by the writings of various pupils, that these favourable changes must be ascribed.

Since the time of Cullen various works, of different degrees of merit, on Food and Diet have appeared. It is unnecessary, however, to mention more than two, both by English physicians;—the work of Dr Paris and that of Dr Pereira. The treatise by the late Dr Paris is a short, compendious, but practical performance, which communicates, in an agreeable form, sufficient information, and no more, on the subject of Diet and its medicinal applications. The in-

formation which the work communicates on certain deranged states of the Alimentary Functions is, it must be allowed, not new or original. But it has the merit of being useful and practical, and it is conveyed in an agreeable and interesting form; and the therapeutic and hygienic directions are in general well founded. First published in 1826,\* it had since that time attained, in 1837, the fifth edition. Differing in some degree, and superior in the application of chemical facts and chemical philosophy, the Treatise published in 1843 by the late Jonathan Pereira,† is most creditable to the knowledge, the talents, and the ingenuity of that physician. Writing subsequently to the researches of Dr Prout, and many of the analyses of Boussingault, Dumas, Einhoff, Mulder, and Liebig, Dr Pereira has been enabled to present more ample, more complete, and more satisfactory views of the relations subsisting between the chemical constitution of articles used as food, and the living bodies which they are employed in nourishing, than had been done by any previous writer. In this department the author has shown extensive and accurate knowledge, and a talent for

\* A Treatise on Diet, with a view to establish, on Practical Grounds, a System of Rules for the Prevention and Cure of Diseases incident to a Disordered State of the Digestive Functions. By J. A. Paris, M.D., F.R.S. London, 1826.

† A Treatise on Food and Diet; with Observations on the Dietetical Regimen suited for Disordered States of the Digestive Organs, and an Account of the Dietaries of some of the Principal Metropolitan and other Establishments for Paupers, Lunatics, Criminals, Children, the Sick, &c. By Jonathan Pereira, M.D., F.R.S., &c. London, 1843.

physiological reasoning, which renders his work one of great utility to the medical reader.

The third great division of the Treatise of Dr Cullen, occupying the whole of the second volume, is devoted to the description of the various Medicines and Medicinal preparations employed in the treatment of Diseases. From the passages already quoted at pages 603 and 604, it cannot be difficult to understand what was the conception formed by Cullen of the requisites of a useful Treatise on Materia Medica. These passages may be regarded as showing, that it was the great, if not the first, duty of an author on Materia Medica, to consider the morbid states of the human body, to inquire in how many of these morbid states there were indications of attempts or spontaneous efforts at recovering from disease, in what cases these efforts required to be left to themselves, in what they required to be directed and aided by the interference of Art, and what was the kind and amount of the interference thus demanded. In this manner he formed what he called Indications of Cure, General and Particular; and by looking principally to the former, he believed that if he arranged Medicinal Agents according to these principles, he should accomplish all that a Teacher of Materia Medica could do. In conformity with these views, he gave a Tabular Arrangement of Medicines in the following manner:—

## MEDICAMENTA QUÆ AGUNT IN

SOLIDA.	{	SIMPLICIA.	{			Astringentia.	.	.	.	Cap. I.		
			{			Tonica.	.	.	.	Cap. II.		
			{			Emollientia.	.	.	.	Cap. III.		
			{			Erodentia.	.	.	.	Cap. IV.		
{	VIVA.	{	{			Stimulantia.	.	.	Cap. V.			
			{			Sedantia; { <i>Narcotica.</i>	.	.	Cap. VI.			
			{			<i>Refrigerantia.</i>	.	.	Cap. VII.			
			{			<i>Antispasmodica.</i>	.	.	Cap. VIII.			
FLUIDA.	{	IMMUTANTIA.	{	{			Fluiditatem. { <i>Attenuantia.</i>	.	.	Cap. IX.		
				{			<i>Inspissantia.</i>	.	.	Cap. X.		
			{	Misturam.	{	{			In genere; { <i>Demulcentia.</i>	.	.	Cap. XI.
						{			<i>Ant-acida.</i>	.	.	Cap. XII.
						{			In specie; { <i>Antalkalina.</i>	.	.	Cap. XIII.
						{			<i>Anti-septica.</i>	.	.	Cap. XIV.
						{			<i>Errhina.</i>	.	.	Cap. XV.
						{			<i>Sialogoga.</i>	.	.	Cap. XVI.
{			<i>Expectorantia.</i>	.	.	Cap. XVII.						
{			<i>Emetica.</i>	.	.	Cap. XVIII.						
EVACU- ANTIA.	{	{	{			<i>Cathartica.</i>	.	.	Cap. XIX.			
			{			<i>Diuretica.</i>	.	.	Cap. XX.			
			{			<i>Diaphoretica.</i>	.	.	Cap. XXI.			
			{			<i>Menagoga.</i>	.	.	Cap. XXII.			

This arrangement has been more or less severely criticized by various authors,—Dr John Murray,\* Dr

\* A System of Materia Medica and Pharmacy. By John Murray, Lecturer on Chemistry and on Materia Medica. In two Volumes. Edinburgh, 1810. Volume First, p. 120. Chapter II.

It is perhaps scarcely worth while to remark, that Dr Murray not only criticizes the arrangement of Cullen with considerable severity, but adds, that “in the system of Brown, which succeeded that of Cullen, more just views were given of the relations of external agents to the living system, and of the laws regulating their action.” (P. 122.) He allows, indeed, that “the operations of Medicines are, even in this system, imper-

Paris,\* and others. It is certainly sufficiently liable to various objections; as what classification is not? The distinctions are founded on various and heterogeneous characters,—physical, physiological, mechanical, and chemical; and it would be easy to show that several of the heads of distinction of one class or order might be arranged, and, in point of fact, arrange themselves under others. Thus it is quite impossible to get over the difficulty, that several Astringent medicines become in certain circumstances Tonics; that many of the family of Evacuants are local Stimulants or Irritants,—for instance, the Errhines, Sialogogues, Expectorants, Cathartics, and Diuretics; that Tonics become Emmenagogues, Sedatives, and Antispasmodics; that Cathartics and Diuretics may become Tonics and Antispasmodics; and, in short, that there is scarcely one member of any one of the classes which may not occasionally, and in particular circumstances, perform the part and duties assigned to and expected from others. Of all these objections,—and there are not a few others which will occur to the mind of every practical physician,—Cullen was aware; and he virtually admits their force in the detailed separate histories which he afterwards gives of each class or order.

fectly explained.” If Dr Murray had attended to the fact, that the system of Brown, as to the operation of Medicines, was copied from Cullen, but rendered more confused, by neglecting some of the distinctions given and conditions prescribed by Cullen, he would have come nearer to the truth.

\* *Pharmacologia*. By John Ayrton Paris, M.D., Cant., &c. London, 1843. Ninth Edition. P. 165, 167.

This table, therefore, we do not submit as either a perfect or faultless arrangement; and all that we allow can be said in its favour is, that it is a convenient classification, better certainly than none; that it has a smaller number of faults than any that preceded it; that it bore a close relation to the physiological principles and the pathological doctrines, which its author inculcated; and that it was for the manner and method, in which Cullen treated the subject of *Materia Medica*, the most suitable that presented itself. It is manifest that, from any tabular arrangement, much benefit cannot be expected. All of them are more or less artificial; and while their principal use is to facilitate the communication of knowledge in teaching, and to aid memory, by giving at one view a synoptical representation of the subject and its divisions, it must never be forgotten, that all these arrangements bear reference to the extent, the accuracy, and the practical applicability of our knowledge of facts, and that as new facts come to light, and new views and theories are deduced from them, all these tables and classifications require to be altered and modified, to be amplified or abridged. The chief recommendation of this table, nevertheless, is this, that it allies itself more closely than any that had preceded it, with physiological principles and pathological distinctions, and comes in this manner to be more directly useful to the philosophical and practical physician. Nor must it be forgotten, that even those writers who, like Dr Murray, found it necessary to criticize the arrangement, bore, nevertheless, unequivocal testimony to its general

merits, by admitting the necessity of arranging medicinal agents according to their known effects on the human body, however they might modify the subordinate divisions. The arrangement afterwards proposed by Dr Murray is entirely of this character ; and similar tacit admissions are given by Schwilguè, Alibert, Barbier, Dr Paris, and several other writers on *Materia Medica*.

Conformably to this tabular arrangement is the method adopted by Dr Cullen in his subsequent detailed history of Medicines, and their uses and applications. In twenty-two successive and separate chapters he gives the history of the medicinal uses and applications of the several individual Medicines which compose the twenty-two orders or classes into which he had arranged the materials of the *Materia Medica* of the time ; considers carefully the circumstances of the human body, and of the diseased states in which it may happen to be, that indicate or contra-indicate the administration of each ; the most effectual and convenient form of administration ; and all those relations, to which the physician has to give attention in the treatment of diseases, and the skilful adaptation of remedies for this purpose.

The general method according to which he treats each class or order is the following :—Each chapter he begins by defining, as accurately as possible, the class of remedies of which he is to treat. He then explains the characters of the whole as a class, showing how they differ from other similar medicines, and wherein they approach and sometimes agree ; and



enumerates all those properties and characters which may be serviceable in explaining their action on the human body, and their operation in curing various diseases. In this manner, Dr Cullen gives ample and instructive general views on the nature and property of Astringents and Tonics, on the differences between these two orders of medicines, and on the points in which they agree; more shortly on the characters and effects of Emollients; fully again on the characters and effects of Stimulants and Sedatives; shortly upon Refrigerants; more fully again upon Antispasmodics; shortly upon Diluents, Attenuants, and Inspissant medicines. Though in all these general disquisitions it is easy to perceive the masterly and judicious hand of the philosophical physician, it is in his delineation of the general characters of Tonics, Stimulants, Sedatives, and Antispasmodics, that the reader recognises the peculiar physiological and pathological principles and reasonings that distinguish all the writings of Dr Cullen. Among the Evacuant class of medicines, he passes slightly over Errhines, Sialogogues, and Expectorants, and examines more fully the characters of Emetics, Cathartics, Diuretic and Diaphoretic Medicines. We cannot, indeed, quit this subject without saying, that these general disquisitions afford the most striking and useful examples of the great talents, which the author possessed, of uniting reasoning and observation, facts and deductions, in forming instructive lessons to practitioners of medicine. We fear that they have never, unless among the reading part of the profession, attracted the attention to which they are entitled.

It is unnecessary, and would be out of place, to enter into any critical examination of the manner in which Cullen has performed the subordinate details of this part of his duty. But it may be proper to advert, in a very general manner, to certain peculiar circumstances, by which the Treatise on Medicines by Dr Cullen is distinguished from all others.

First, it may have been remarked that, previous to the time of Cullen, and during even his days, it was a general custom among physicians to classify, distinguish, and designate Medicines according to certain supposed properties and powers which they were believed to exert in curing certain diseases, sometimes in removing or alleviating particular symptoms, with little or no reference to the morbid condition of the body on which these disorders and symptoms depended. Thus they had Cephalic and Nervine Medicines for the Brain and Nerves; they had Antiphthisical Medicines against Consumption, Bechics against Cough, Antidemics against Giddiness; they had Cardiacs or Cordials against Fainting Fits; Antihysterics, Antispasmodics, Alexiterics, and whole tribes of medicines distinguished and designated upon similar principles. All this symptomatic and hypothetical system of classifying Medicines Cullen abandoned as unprofitable, and resting upon fallacious foundations. He perceived that in many, if not in all these instances, names were given, and qualities assigned, which merely concealed ignorance; and he adhered to the simple and intelligible method of arranging medicines, either according to their obvious sensible properties and

effects, or to their physiological action, as ascertained by observation and experiment, on the human body ; and in certain cases, according to physical, mechanical, or chemical action, manifested by unequivocal effects. He knew that we possessed not one single medicine which could be called antiphthisical or antihysterical ; no remedies which exerted specific action upon cough, giddiness, pains, fits of fainting, or of epilepsy ;\* and that the only mode in which the physician can act upon these symptoms is by acting on their pathological cause or causes, and through the medium of agents which produced effects on the system at large. This it was which led him to maintain the necessity of establishing what he called General Indications, and considering the means by which these indications were to be answered and fulfilled.

In the second place ;—it was a fancy in those days that it was possible to apply to every morbid state, and every disease, a direct remedy. Cullen did not at that time go so far as to say that there was no direct remedy for any morbid condition ; but the tendency of his writings was in this direction ; and by his explanations of the pathological causes of diseases,—above all, by his pathological distinctions,—

\* “ *Medicos tandem tædet et pudet,*” says Dr Gregory, in the exact spirit of Cullen, “ *diutius garrere de remediis specificis, et alexipharmacis, et febrifugis, et cephalicis, et ophthalmicis, et cardiacis, et stomachicis, et restorativis, aut de antiphthisicis aut antihydropicis, aut antiparalyticis, nisi eorum naturam et modum quo prosint, quodammodo ostendere et explanare possint.*” *Conspectus Pars altera, Cap. XXIV. DCCCCXLVII.*

he taught physicians to think and reason in such a manner, that this principle, which is not fully understood even in the present day, is, nevertheless, gradually becoming more evident to physicians. Closely allied, no doubt, with the establishment of this principle, was his admission of the Conservative and Sanative Powers of the System; (*Vis Naturæ Conservatrix et Medicatrix*);\* and had he carried this a little further, and maintained more generally the spontaneous curability of various diseases, this would have placed in a clearer light the comparative powers of Nature and the powers of Medicines in the cure of Diseases; and the allied principle thence resulting, that it is by indirect means only, that physicians can be said to cure diseases. It were unreasonable, however, to expect from any one individual that he is to accomplish everything. It is enough that Cullen pointed out a better and a safer mode of inquiry, showed what errors were to be avoided, and what course was to be pursued in attaining a proper knowledge of the uses and applications of Medicinal agents.

Thirdly;—We have already spoken of the severe logical method, and the skeptical or inquiring spirit, with which Cullen treated the First Division of his subject, namely, that of Aliments. Not less conspicuous, as they were more necessary, were these characters in the Second Division on Medicines. Cullen had been called speculator, hypothetical writer, sys-

\* *Materia Medica*, Part II., Chap. V., p. 140; Chap. VI p. 222.

tem-builder, and similar names not very becoming, even had there been reason for their application. It has been already shown, in various parts of this work, that these characters were not justly applicable. They originated, indeed, with persons who either did not read, or did not understand his writings. They had certainly overlooked and forgotten the numerous instances of painstaking and careful generalization of facts, which his writings everywhere presented. In the *Materia Medica*, and in the part on Medicines, his procedure is so remote from speculation and hypothesis, that he appears entirely as a skeptical inquirer. He denies, doubts, and calls in question every statement not well substantiated. He even goes somewhat in the opposite direction; and if he does not deny, he at least scarcely admits statements made by others on reasonable authority. Thus he states that he never could give Nitrate of Potass in the large doses recommended by Dr Brocklesby,\* though, since the time of that physician, Nitre has been administered, not, indeed, to the large amount practised by Dr Brocklesby upon young soldiers,† but certainly in larger doses, and more continuously, than it previously had been. In like manner, he threw doubts upon the practice of Dr Lind of Haslar Hospital as to the propriety and safety of giving

\* *Materia Medica*, Part II., Chap. VII., p. 343.

† There is reason to believe that some fallacy took place in the cases reported by Dr Brocklesby. The remedy was given in Military Hospitals to Soldiers; and we all know what deceptions were carried on in these Institutions at the time at which Dr Brocklesby practised. It is not unlikely that much of the Nitre never went into the stomachs of the patients.

Opium in the time of the hot stage of intermittents,\* though that physician, and several of his adherents, represented the practice to be perfectly free from danger, and highly effectual.

All this skepticism or inquiring doubt, nevertheless, was highly useful in impressing the lesson, that all statements a little singular regarding the effects and applications of medicines ought to be received with caution, and, in certain circumstances, with suspicion.

In the fourth place ;—Notwithstanding all his care in exposition, in illustration, and in the specification of conditions, and limiting, exceptional, and modifying circumstances, Dr Cullen did not, in his statement of doctrines, physiological and therapeutic, altogether escape censure and criticism, even at the hands of those from whom a different and more liberal and candid course might have been expected. Several instances of this took place; but only one can I here notice. It has been already stated that Dr Murray, the author of an excellent system of *Materia Medica*, blamed the general arrangement of Cullen, as one “resting on principles nearly altogether false;”† that “the assumption that some medicines act exclusively on the solids, others on the fluids of the body, is incorrect, for, with the exception of two or three classes, the action of the whole is on the living solids.”‡ This criticism Dr Murray repeated in a

\* *Materia Medica*, Part II., Chap. VI., p. 237.

† *A System of Materia Medica*. By John Murray, &c. Edinburgh, 1810. Chapter II., Vol. I., p. 120, 121.

‡ *Ibid.*, p. 121.

stronger and more detailed shape in a subsequent part of his work, when speaking of the general action of Narkotics, and the vexed question of whether they possess and evince in their operation a stimulant power.

Dr Murray had himself in a great degree prejudged this question, and assumed the existence of the stimulant property; for he had arranged Narkotics and Antispasmodics as diffusible stimulants, under the head of General Stimulants; and after giving the general definition of Narkotics as “substances which diminish the actions and powers of the System without occasioning any sensible evacuation;” he adds, “This definition is imperfect, inasmuch as it does not include that stimulant operation which they equally produce, and which, in part at least, must be admitted as the cause of these effects.”\*

Dr Murray then states and contrasts the theory of Cullen, and that of Brown, in the following manner.

“Their natural tendency (Narkotics) was supposed (by Cullen) to be to depress the powers of life; if given in a large dose, this power was exerted with effect, and hence arose symptoms of exhaustion; but if given in a smaller dose, the *Vis Medicatrix*, or Preserving Power, was enabled to resist, and, by its resistance, occasioned the symptoms of increased action that first appeared. These substances, therefore, were considered as directly Sedative and as indirectly Stimulant.

“Precisely the reverse of this view was advanced

\* A System of Materia Medica. By John Murray, &c. Vol. I., Chap. III., p. 134.

by Brown; Narkotics being regarded as Stimulants, surpassing all others in the diffusibility and little durability of their action; and, on this principle, their effects were explained in the following manner.

“ It is the necessary effect of Stimulant operation to produce for a time increased action; but as this is attended with a diminution of Vital power, the excitement soon ceases, and diminished action succeeds. These effects are proportional, partly to the absolute force of the exciting power, and partly to the rapidity with which it operates. If sufficiently strong, and if at the same time it be diffusible and transient in its operation, the excitement it produces is quickly raised to its highest point, and is as quickly followed by proportional languor and diminished action. Or if the dose is large, the stimulant effect is so rapid as to be hardly perceptible, and hence the sedative or depressing effects only appear. Thus Narkotics were regarded as powerful stimulants, whose action is not confined to the part to which they are applied, but is rapidly extended over the system. In a moderate dose, they promote action of every kind, which is succeeded by a degree of languor and debility proportioned to the excitement that had been raised; and in a large dose, they produce diminution of power, and consequently of action, without any symptom of previous excitement. Hence they were regarded as directly stimulant and indirectly sedative.

“ If in investigating this subject,” continues Dr Murray, “ we merely contrast these two theories, little doubt can remain of the superiority of the latter.



The former is founded on a hypothesis established by no evidence, that a power presides over the system, ready to resist every noxious application; the latter is apparently more strictly deduced from the properties of the substances whose operation is to be explained; for, as it is proved, and indeed admitted, that the stimulant operation resulting from the exhibition of Narkotics follows immediately, and previous to any symptoms of languor and debility, these ought strictly to be considered as the consequences of the former.”\*

Now, we admit at once that the idea of the *Vis Conservatrix et Medicatrix* is an hypothesis or assumption; though it cannot be justly said that it has in its favour no evidence; for there are too many indications that there is in living bodies some counteracting agent which resists, or at least seems to resist, hurtful actions in whatever mode caused.† And we further admit that Dr Cullen, in his general statement of the properties and effects of Narkotics, represents them to act as direct Sedatives and indirect Stimulants.‡ We cannot, however, allow that Cullen was not aware of, or had not remarked, the previous stimulant operation in many of these substances. “There is, however, here,” he observes, “a considerable difficulty occurring, as it is to be particularly remarked, that Narkotics, in-

\* A System of Materia Medica. By John Murray. Vol. I., Chap. III., pp. 138, 139, 140. Edinburgh, 1810.

† A Treatise of the Materia Medica. By William Cullen, M.D. Part II., Chap. V., p. 140; Chap. VI., p. 296. On Camphor.

‡ Ibid., Part II., Chap. VI., p. 222.

stead of proving always sedative, or diminishing the action of the heart, they very often seem to be powerfully stimulant with respect to this, and, in their first operation, often increase the force and frequency of its action.”\*

It is unnecessary to advert to the mode in which Cullen explains, or tries to explain, this stimulant operation, which, though ingenious, can scarcely be called satisfactory. The facts we take as he gives them, and we attach little importance to the explanation. He afterwards, when speaking of Opium, from which, indeed, most of his inferences were deduced, remarks;—“ But I have observed above, that Narcotics, and particularly Opium, in its first operation, often irritates the sanguiferous system, and excites the force of the circulation. And however we may dispute about the causes of this, the fact is certain, and that this, in a certain degree, gives Opium the powers of a Cordial and Exhilarant. On this occasion, it is to be observed, that though the action of the stimulant should not entirely prevent the sedative power, it often puts it off for some time, to give what we may call the intermediate state of Ebriety, which, according to the balance between the stimulant and sedative power, may appear more or less, or subsist for a longer or shorter time, and may therefore more particularly explain the operation of Opium in different persons and cases.”†

\* A Treatise of the Materia Medica. By W. Cullen. Part II., Chap. VI., p. 220.

† A Treatise of the Materia Medica, Part II., Chap. VI., pp. 229, 230.

The limits within which these observations must be restricted, allow me not to follow Dr Cullen in the various passages in which he distinctly shows, that he was quite aware of this stimulant action, not only in Opium,\* but in other substances, as the Cherry Laurel Water, p. 283; Wine, 316. To me it appears that Dr Murray has scarcely considered, with the attention to which they are entitled, the histories given of the particular Narkotics; and he certainly assigns to Dr Brown a larger share of merit than justly belongs to him, when he contrasts his hypothesis of the action of Narkotics with that of Cullen. The question may be justly asked, Does not Brown borrow from Cullen this very character of the previous stimulant action of these agents?

I shall conclude these remarks by one quotation from Cullen, which affords a sort of key to the explanation of this strange attempt at misrepresentation. After explaining the most eligible method of employing Opium and its preparations in the treatment of Fevers, and the precautions and limitations which ought to be observed in their administration, Dr Cullen delivers the following expression of his sentiments:—

\* See particularly under the head of Inflammatory Diseases. “But Opium, on many occasions, as we have already said, is a stimulant power; and whoever denies this, as some in writing have done, appears to deny and misrepresent facts admitted by everybody else.” *Materia Medica*, Part II., Chap. VI., p. 237.

The only point on which there is reason for doubt is the question of the action of the Cherry Laurel Water, which modern experiments seem to show acts as a direct sedative; paralyzing the action of the heart, yet at the same time acting on the brain and spinal chord.

“ Many are fond of a more free use of Opium than had been common before, and have believed that this was introduced by a certain noted teacher and author; but I assert that I myself was the first who freely and largely employed opium in fevers, under certain restrictions, indeed, which, neglected by other practitioners, have occasioned much mischief.”\* Upon the propriety or impropriety of administering opium in Fevers, this is not the place nor the time to give any opinion. But the passage shows the unscrupulous character of the claimant for several of the doctrines and precepts of Cullen; and as Opium, Alcohol, and Wine were the principal agents in the *Materia Medica* of Brown, it is not difficult to understand how eager he and some of his partizans had reason to be, in ascribing to Dr Brown any reputation that might arise from this supposed improvement as to the theory of their mode of action.

An objection, which some have pronounced to be serious, has been urged against the *Treatise* of Cullen, in the disregard avowed and expressed by him of the aids of chemical analysis in illustrating the physiological and the therapeutic powers of medicinal agents. This objection we willingly admit. But we add, that it was certainly excusable, and in some degree to be defended, when we remember that, at that period, general chemistry was in a very infant state, and that the analysis of organic bodies was not only imperfect, but positively erroneous. It has been shown how little benefit the knowledge of che-

\* *Materia Medica*, Part II., Chap. VI., p. 235.

mical analysis at that time rendered to the explanation of the action and comparative nutritious powers of different articles of food. To the elucidation of the action of medicinal agents, Chemistry, as then taught, was still less capable of rendering beneficial service. Cullen was thus placed in the necessary and unavoidable difficulty of either adopting what was erroneous and not quite trustworthy, or of rejecting that assistance, which after all might have been fallacious, and was certain to be useless. It may be allowed that he acted wisely, therefore, in not burdening his work with information of questionable merit, and information which subsequent experience has shown must have become in the course of a few years of no value whatever.

The disregard expressed by Cullen for the chemical analysis of medicines belonged to the period when he wrote, and is no longer well founded. The knowledge acquired in the course of twelve or fifteen subsequent years tended greatly to diminish, and finally to remove, the grounds of that disregard. That knowledge was applied with judgment and diligence by various able pharmacological writers in this country and abroad, soon after the commencement of the present century. The Dispensatory of Dr Andrew Duncan the second, and the Elements of Dr John Murray, were both remarkable for showing how much might be effected by due attention to the chemical constitution of medicines. The Treatises of Henry and Guibourt, the Dictionary of Merat and De Lens, and, subsequently, the Dispensatory of Dr Christison, and the elaborate and learned work of

the late Dr Pereira, supply all the requisite information upon this as upon several other departments of *Materia Medica*.

But in making the concessions now stated in favour of chemical knowledge, as applied to the elucidation of the action of medicines, it must be further admitted, that the results of this application tend rather to confirm than weaken the correctness of the opinion maintained by Dr Cullen. Notwithstanding the information which the labours of between fifty and sixty years have produced, it cannot be said that the knowledge of the action of medicinal agents has been thereby proportionately enlarged and rendered accurate. It is important and desirable to possess as correct knowledge as possible of the chemical constitution of Medicines; but that knowledge, however accurate, is still insufficient to guide the physician. The truth is, that Therapeutics and *Materia Medica* rest, like all other branches of medical science, upon observation and experiment;—observation of the effects of medicines upon the living body, and where that fails, upon experimental trial. We know that Opium contains Morphia, Narkotine, Codeine, and Meconates; but we must further ascertain, by observation and experiment, the peculiar effects of these principles upon the animal body. No information that chemical analysis could have furnished of the constituent principles of Foxglove could have told us, without actual trial, that this drug has the effect of diminishing the number of the cardiac beats in a given time, or produced giddiness and faintness, and increased the amount of the urinary secretion.

Upon the whole, we are strongly persuaded, that to any one who peruses the work of Cullen attentively in all its divisions, not in individual parts, and compares what he states in the detailed histories with the statements made in the general dissertations,—in short, takes a candid and enlarged view of the entire work,—it will appear, that the alleged mistakes and errors are fewer and more trivial than might have been expected in a work devoted to the entire subject of Aliments and Medicines, considered upon principles so new and so little employed previously.

We pretend not to say, nevertheless, that the work is altogether free from some faults, both in arrangement and in details. Some of the former have been already referred to. It appears, further, that the junction of Narkotics with Refrigerants, under the general head of SEDATIVES, rests upon principles neither sound in theory nor useful in practice. The author was mistaken in the pathology of Dysentery, and in the theory of the operation of Opium (p. 244), and of Ipecacuanha (p. 477), in that disease. For it was not by determining downwards, and by any laxative operation, that these agents produced benefit, but by abating inflammation and irritation, and abating concomitant fever, that they acted either alone or conjointly. It were desirable also, that Cullen had not mentioned in his work so absurd a mode of preparing Tar Ointment as that of roasting a leg of mutton and basting it with Tar. (P. 185.) What advantage such a mode of preparation can have it is not easy to discover. It is unnecessary and frivolous, however, to notice these slight and rare blemishes in a

work so extensive, and with so many excellences. More profitable and more agreeable will be the duty of enabling readers to form a just estimate of the merits of the treatise, of the benefit which it was calculated to render, and which it actually rendered, to Medicine as a Science and as an Art, and of the new and important instructions which it taught to physicians upon the nature and operation of remedial agents. On this point I have already in some degree touched; and the little that it is necessary to add I shall endeavour to make as brief as possible.

The great service which the Treatise on Materia Medica by Dr Cullen rendered, consisted in establishing the principles and rules of this department of Medicine upon the foundation of Physiology and Pathology as perfect as the time admitted. He did not take Medicines and medicinal preparations, and say, What is this good for, and what does that accomplish? He looked to the morbid conditions of the human body, and proposed the question, How are these to be alleviated, removed, and cured? what are the means and methods to be employed for these purposes? what agents are to be used, and what are to be avoided and rejected? His general purpose throughout was to establish between pathological conditions and therapeutic methods and agents a connection as direct and rational as knowledge at that time permitted. He perhaps might not, in all instances, be successful in this object. But he taught others, who might follow him, what ought to be their endeavour. Cullen had in short realized, in as great a degree as circumstances permitted, the conceptions



of Thomas Willis, and had accomplished what that ingenious and experienced person had seen was necessary, but had been unable himself to effect.

The beneficial influence of the Treatise by Cullen was seen, not immediately, indeed, but in the course of from eight to twelve years. A Translation of the Work into the French language, by M. Bosquillon, appeared at Paris in 1790, very speedily after the work was published in England;\* and, in the course of the same year, appeared at Leipzig two Translations into the German language; one with annotations by Samuel Hahnemann, then a young physician little known, but subsequently distinguished as the founder and propagator of the doctrine of Homœopathy;† the other with an appendix by George W. Chr. Consbruch.‡

\* Cullen, W., *Traité de Matière Medicale*, traduit de l'Anglais par Bosquillon. Paris, 1789–1790. Deux volumes 8vo.

† Wilhelm Cullen's *Abhandlung über Die Materia Medica*, Nach der nunmehr von Verfasser selbst ausgearbeitet Originalausg. Übersetzt und mit Anmerkungen. Von Samuel Hahnemann. 2 Theile gr. 8vo. Leipzig, 1790.

‡ Wilhelm Cullen's *Materia Medica*; oder Lehre von dem Nahrungs-und Arzneimitteln. Aus dem Englisch. Zweite Auflage. Mit Zusatz. von Georg. W. Chrph. Consbruch. Gr. 8vo. Leipzig, 1781 und 1790.

I must here observe, that the Translation by George W. Consbruch must have been made from the early and imperfect edition of the *Lectures on Materia Medica*, published in 1771; for it is announced as a second edition, and the first is stated to have appeared at Leipzig in 1781. This merely shows the anxiety of German readers to know the writings of Cullen. Consbruch was afterwards engaged with John Christian Ebermaier and J. Friedrich Niemann in the publication of an *Encyclopædia of Medicine and Surgery*, which continued from 1817 to 1830. The work is voluminous and elaborate, con-

A translation into the Italian language, by Angelo Dalla Decima, appeared between the years 1792 and 1800, in six volumes, at Padua.

Original Treatises on Materia Medica appeared in France in 1805, by Schwilgué;\* in 1804, 1808, 1812, and 1817, by Alibert;† in 1810, 1811, 1819, and 1824, by Barbier;‡ all of which were formed more or less closely on the model of the English author. It were desirable that these authors, and several other foreign physicians, had adopted a greater degree of the skeptical and inquiring spirit of Cullen; for the principal and most obvious objection which the English reader finds in them is the excessive confidence in various remedies, especially from the Vegetable Kingdom, which they evince, and which it is by no means easy for the practical physician to admit as well-founded.

It would be out of place in these pages to attempt

sisting of eleven parts; and embracing almost every division of Medical Science, and even Veterinary Medicine, was long in considerable esteem. In what estimation it may be held since the publication of more recent works, devoted to particular departments of Medicine, it is unnecessary to say.

\* *Traité de Matière Medicale.* Par C. J. A. Schwilgué, D.M. Deux Tomes, 8vo. Paris, 1805. Troisième Edition, 1818. Par P. H. Nysten.

† *Nouveaux Elemens de Therapeutique et de Matière Medicale.* Par J. L. Alibert. Deux Volumes 8vo, 1804; 2<sup>d</sup>e Edit., 1808; 3<sup>ieme</sup> Edition, 1812; 4<sup>ieme</sup> Edition, Revue, &c. Paris, 1817.

‡ *Traité Elementaire de Matière Medicale.* Par J. B. G. Barbier, Medecin Ordinaire de L'Hotel Dieu d'Amiens. Trois Tomes. T. 1 and 2. Paris, 1819; 3<sup>ieme</sup>. 1820. Seconde Edition avec des Augmentations et Changemens. Trois Tomes. Paris, 1824.

to give any character of these works, or to trace their respective merits, unless in so far as their method of treating the subject bore some relation to that observed by Cullen. The work of Schwilgué, which was first published in 1805, is that which least shows the impression of the doctrines taught at the close of the eighteenth century. But it cannot be said to be without impression of this kind; and the Treatise was republished after his death, which took place in February 1808, first in 1809, and several years afterwards, with notes and various useful additions and corrections by Pierre Hubert Nysten, the industrious follower of Bichat in experimental Physiology.

The New Elements of Therapeutics and Materia Medica, by M. Alibert, is in many respects a praiseworthy book. Proceeding upon Physiological and Pathological principles, and extensively acquainted with the writings of physiologists and physicians, M. Alibert has placed before the foreign medical reader more just and sound views on the relation between morbid states and remedial agents than had previously been seen in France. It is true that he assigns to Barthez, to Cabanis, to Legallois, to Chaussier, and to Orfila the entire merit of any improved and scientific views which had been formed at the beginning of the nineteenth century on the action of remedies,\* and mentions Cullen occasionally, only to controvert and criticise his views and reasonings.

\* Nouveaux Elemens de Therapeutique et de Matière Médicale. Tome I. PROLEGOMENES throughout; especially XX., XXI., XXIV., XXV.

But this is a small matter, certainly not more than was to be expected. We admit to the fullest extent the beneficial influence of the reasonings, the teaching, and the writings of these eminent men upon the progress of Medicine as a science and an art. We are even willing to allow, that if Cullen had not existed, or had not written, various physicians and physiologists in France and Germany might have contributed, during the concluding ten years of the eighteenth century, to form the sound and just views on Disease, its causes, and the methods of treatment, which he certainly had the merit of bringing forward. The human mind is never stationary. What is not effected by thinking and inquiring men in one country is in general accomplished by individuals of similar habit of mind in another. This, however, is not the question at present under consideration. This is, What is the nature and amount of the influence exercised by the *Materia Medica* of Cullen on the Therapeutic and Pharmacological treatises and writings of the nineteenth century? And to any one who takes an enlarged and just view of the subject in its different bearings, we think it must be manifest, that the Treatise by Cullen had a great and powerful influence upon the writings which were subsequently published; and, in the particular case of the work of M. Alibert, it must be manifest to any one who peruses it carefully, that in whatever manner this has been effected, the doctrines and reasonings of Cullen are inseparably mingled with all his therapeutic principles and applications. Alibert set the earliest example of the methodical enumeration of

the chemical qualities and the physical and physiological effects of medicines, and gives full accounts of the therapeutic uses of each. The work is also clear, methodical, accurate, and highly instructive.

Next in order came the work of Barbier, being first published in 1810, in one volume octavo. This was followed in 1811 by a Treatise on Hygiene and General Therapeutics in two volumes; and the whole were incorporated in three volumes in 1819–20 and 1824. This is a book of more pretension, greater learning, more profound views in Physiology and Pathology, and in some respects more elaborate and complete in their application to Therapeutics and Materia Medica than that of Schwilgué, and rather more elaborate than that of M. Alibert. In the Second Edition, which appeared in 1824, much new chemical information is introduced.

The German States, with their characteristic activity, produced many works on Materia Medica, varying in merit. Some, as the Manual of E. Horn, in 1805,\* were upon the principles of Dr Brown; others, as the more elaborate system of C. H. Pfaff, in 1808,† upon Chemical principles; and others again, as those of F. N. C. Gren (1813),‡ Ph. Hartmann (1816),§ G.

\* E. Horn Handbuch der Praktischen Arzneimittellehre Zweite Auflage. Berlin, 1805.

† C. H. Pfaff System der Materia Medica. Leipzig, 1808.

‡ F. A. C. Gren Handbuch der Pharmakologie. Dritte Auflage. Halle und Berlin, 1813.

§ Ph. C. Hartmann Pharmacologia Dynamica. Viennæ, 1816.

Voigtel (1816),\* Fr. Jahn (1818),† J. Arnemann,‡ W. Vogt (1821–1823),§ and Carl Sundelin (1825),|| more upon Physiological and Pathological principles. Most of these works went through several editions; that of Pfaff, the most elaborate, was republished, with all the most recent information, in 1821 and 1824. To these may be added a well-arranged and short system of Pharmakology and general Therapeutics, given in the eighth, ninth, and tenth volumes of the INSTITUTIONES MEDICÆ¶, composed between 1806 and 1810 by Kurt Sprengel, Professor at Halle in Saxony, and the learned author of the Pragmatical History of Medicine. Of these Treatises it may in general be stated, that after the first five or six years of the present century, the principles taught by Dr Brown were quietly set aside; and, in a similar manner, those inculcated by Cullen were adopted and recognised. Kurt Sprengel, Hartmann,\*\*

\* G. Voigtel Vollständiges System der Arzneimittellehre, Herausgegeben von Kühn. Leipzig, 1816.

† Fr. Jahn, Prakt. Materia Medica, Vierte Auflage. Erfurt, 1818.

‡ J. Arnemann, Prakt. Arzneimittellehre; 6 Auflage. Gott., 1819.

§ W. Vogt Lehrbuch, der Pharmakodynamik. 2 Bände. Giessen, 1821–1823.

|| Handbuch der Speziellen Heilmittellehre, von Carl Sundelin, D.M., und Ärzte des Medizinisch-Klinisch Institut der Universität zu Berlin. Zwey Bände. Berlin, 1825.

¶ Kurtii Sprengel, M.D. et P., Institutiones Medicæ. Leipzig, 1810. Paris, 1810. Mediolani, 1816–1817. Volumina XI., 8vo.

\*\* Phil. Caroli Hartmann, M.D., et in Scientiarum Universitate Vindobonensi, C. R., Olim Professoris, P. O. Therapia

and Sundelin are the principal agents in this return to sense and judgment.

In the Italian Peninsula the Treatises on *Materia Medica* during this period referred to are not sufficiently important to justify any special mention in this place.

Testimony of a very decided character to the merits of the Treatise on *Materia Medica* as a useful practical work, was rendered by various English writers; but particularly by two,—Dr James Gregory, the successor of Cullen, first in the Chair of Theory, and afterwards in that of Practice of Medicine; and by Dr Woodville of London, author of the instructive work on Medical Botany. Dr Gregory, in his *CONSPECTUS* of Theoretical Medicine or Institutions, composed the second part of that work on General Therapeutics very closely indeed upon the model of the Treatise on *Materia Medica*. Dr Gregory has with great learning and judgment, and with logical clearness and philosophical precision very rare in medical writings, shown what a useful Treatise on General Therapeutics ought to be. He has certainly improved greatly upon his model, by condensing and abridging the materials; but the great merit of this part of the *CONSPECTUS* is, that while it presents simple, clear, and intelligible views of the principles of General Therapeutics, the reader nowhere recognises want of information, but in every division finds the instruction which he feels to be necessary. The observations everywhere made in this work on the proper applications of Medicines, and the limits

*Generalis, secundum Prælectiones Publicas Edita a Medico Practico. Lipsiæ, 1835.*

within which their use ought to be confined, on the influence of Diet, Exercise, and Temperance, show the mind of the rational and philosophical physician, and may at all times be studied with advantage. The work in this completed form was first published in 1782 and in 1790, and continued long to be a classical guide to the students of the Edinburgh School.

The Delineation of General Therapeutics given by Dr James Gregory stands almost alone as a logical and philosophical view of the subject,—ample without being prolix, succinct without being obscure. The only performance by a foreign author that makes any approach to the sketch of Dr Gregory is that which is given by the learned Kurt Sprengel, of Halle in Saxony, in the eighth, ninth, and tenth volumes of his INSTITUTIONES MEDICÆ, published first at Halle and Leipzig in 1809; again at Paris between 1810 and 1819; and at Milan in 1817. The eighth and ninth volumes of the Milan edition form the *Pharmacologia*, and give a complete view of that subject for the time in which it was written. The tenth volume forms the General Therapeutics, and gives a delineation of that subject, to which must be assigned the praise of being luminous and correct, logical and instructive.\* The reader nevertheless looks in vain for the strong clear sense of the Scottish author, the combination of judgment and decision, with which he determines doubtful points and discards useless inquiries, and the happy dexterity with which he presents to the reader the topics, which

\* Curtii Sprengel Institutiones Medicæ. Mediolani, 1816–1817. Volumina Undecim. 8vo. Reimprimebat J. Silvestri.



it most closely concerns him to know. In some respects Sprengel shows a greater degree of the Academical professor, the learned author whose labours had methodized and elucidated the History of Medicine. Gregory, on the other hand, appears as the practical physician, the experienced and judicious observer, who knows what remedies can, and what they cannot accomplish.

While Dr Gregory produced, in a condensed shape, the general Therapeutic doctrines, the didactic method, the subjective arrangement, and the most important practical applications of the Treatise of Dr Cullen, Dr William Woodville of London, an esteemed pupil of Cullen, published a work, in which he had constant occasion to make known the facts recorded and inferences deduced by the Edinburgh Professor.

Dr Woodville published his work on Medical Botany first between the years 1790 and 1793, in three volumes quarto, with a fourth as supplement in 1794; and a second edition of the same appeared in 1810.\*

A valuable part of the work of Dr Woodville was the judicious and careful estimate which he formed

\* Medical Botany; containing Systematic and General Descriptions, with Plates of all the Medicinal Plants, Indigenous and Exotic, comprehended in the Catalogues of the *Materia Medica*, as published by the Royal College of Physicians of London and Edinburgh, &c. By William Woodville, M.D., &c. In three volumes 4to. 1790. Supplement, London, 1794. Second Edition, Four Volumes 4to. 1810. Third Edition, with Additions and Corrections. By Sir William Jackson Hooker. London, 1832. Five Volumes 4to.

of the physiological properties and of the various therapeutic powers in plants and vegetable articles employed in Medicine; and for this he was mainly indebted to the Treatise of Dr Cullen, whose authority is quoted under almost every article, and from whose work, as in the case of Tobacco, Camphor, Opium, Tea, and several other substances, Dr Woodville gives ample extracts. In truth, it may be justly said, that the medical and therapeutic, or pharmacological part of the Medical Botany was in general that of Dr Cullen. This frequent reference to the authority and experience of Cullen tended, undoubtedly, to increase the utility and exalt the authority of the work of Dr Woodville.

This was for many years a standard Treatise on Medical Botany, and may still be perused with advantage and instruction. Since the appearance of the large and more elaborate works of Friederich Gottlob Hayne\* and Th. Fr. Nees Von Esenbeck,† it has lost something perhaps of the high character, which it first acquired; partly in consequence of the more accurate state of Botanical science, and greater precision in Botanical characters, introduced by

\* *Getreue Darstellung und Beschreibung der in der Arzneykunde gebräuchlichen Gewächse, wie auch solcher welche mit ihnen verwechselt werden können.* Von Friedrich Gottlob Hayne. Berlin. R. 4to. XI. Band, 1805.

† *Plantæ Medicinales. Oder Sammlung Offizineller Pflanzen. Mit Lithographischen Abbildungen, u. s. w.* Von Dr Th. Fr. L. Nees von Esenbeck, Professor der Pharmacie auf der Rheinuniversität. u. s. w. M. Weyhe, u. s. w. Dusseldorf. Band I. 1829, Band II. 1828. Erster Supplement-Band mit 120 Tafeln. Dusseldorf, 1838. Gr. Folio.

Bonpland, Persoon, Robert Brown, Sir James Edward Smith, Decandolle, and the two Hookers,—partly in consequence of the defects inseparable from any first work on the subject published at that time. But a Third edition of the work of Dr Woodville, with considerable additions and important rectifications, forming five volumes, was published at London in 1832, by Sir William Jackson Hooker, and places that work, as nearly as may be, on a level with those of Hayne and Nees von Esenbeck.

In the year 1797, eight years after the publication of the Treatise of Cullen, appeared anonymously the first part of a volume, of which Dr Richard Pearson of Birmingham avowed himself to be the author, by publishing, in 1808, the whole work in a complete and revised form. This Treatise, entitled *A Practical Synopsis of the Materia Alimentaria and Materia Medica*, which is accordingly divided into two parts, is remarkable for giving short and compendious, yet clear and useful accounts of the dietetic and nutritious properties of the various articles used as food and drink, and of the physiological and therapeutic effects of the medicinal agents employed in the cure of diseases, as these effects were ascertained by the most able and credible observers. In ascertaining and declaring the weight of evidence on the latter point, Dr Pearson appears to have held the balance with a steady hand, and estimated its oscillations with an accurate eye. He has introduced much valuable information from the elaborate work of Murray of Goettingen, and all the important materials which had been collected during the experience of

nearly forty years by the most eminent English and foreign clinical practitioners and experimentalists. He has even referred for various important facts to the best original inquiries and the most able monographs; and his work not only presents a comprehensive and just view of the state of the science of *Materia Medica* in 1808, but constitutes almost a Digest of the Literature and Annals of the Art.

Dr Pearson has rather depreciated the merit of the physiological and pathological disquisitions introduced by Dr Cullen, and has pronounced upon them a judgment more severe than the circumstances justified. It would be easy to show that these disquisitions are most important parts of the work of Cullen; and that without them that work would have been imperfect, and in many respects obscure, and not quite intelligible. A more judicious critic would have said, that though in the *Treatise* of Cullen they were necessary, in a *Manual* or *Compendious Synopsis*, such as that planned by Dr Pearson, they were out of place, and might without inconvenience be omitted. The object of Dr Pearson was to present, in a compendious and portable shape, all the most important information contained in Cullen's work, and in others, which the experience of twenty years had placed at the disposal of the physician; and this he must be allowed to have ably accomplished.

Dr Pearson has often differed from Cullen in the degree of confidence which is to be reposed in various remedies. Some medicinal agents, of which that author had formed too high an estimate, he has re-

duced to a more humble rank ; a few, which Cullen has unjustly depreciated, he has seen occasion to raise to a more just position. In these changes Dr Pearson has been guided both by personal observation and by the concurrent testimony of many respectable observers.

Other testimonies to the merits of the Treatise on *Materia Medica*, further proofs of the influence of that work and its characteristic doctrines, I might yet adduce. This, however, is unnecessary. One only I shall add from Dr John Mason Good, who, though often led, from peculiar views and opinions on Nosology and Pathology, to differ from Cullen, and though in vigour and force of intellect greatly inferior, must in this case be held to be an impartial witness, and a judge of sufficient competency.

Dr Mason Good, in the Preface to his “ Study of Medicine,” has remarked that the First Lines of Dr Cullen, when read as they were delivered, in connection with his Treatise on the *Materia Medica*, constitute the most important course of instruction, that has ever perhaps been laid down and completed by the same individual. “ But for this purpose,” he adds, “ they must be read together, though they were not published together, nor for the express design of forming a contemporaneous study ; for it is a singular fact, that the First Lines of the Practice of Physic, though full both of mind and of matter, of elaborate axioms and theoretical principles, contain little of what the title suggests ; while the Treatise on the *Materia Medica*, without making any pretensions to the subject, is altogether a prac-

tical work, replete with practical principles, and founded upon a practical investigation."

To examine critically and logically the opinions here expressed is scarcely worth while. The judgment is vague and indiscriminating, and does not precisely distinguish the characteristic merits of the Treatise by Dr Cullen. But the opinions may be taken as the testimony of one who, as aiming at distinction in the same line of scientific and professional exertion, might be supposed to have acquired some title to express an opinion, though on the writings of a physician, who was in all respects much his superior. The opinion expressed on the propriety of studying the Treatise on Materia Medica along with the First Lines, with which we are at present concerned, is that which in all probability will be shared by all the most competent judges, who have studied the work with greatest attention. And it is not to be forgotten, that this idea it was, which, in some degree, though not derived from Dr Mason Good, induced Dr Thomson, in his edition of the First Lines, published in 1827, to introduce, from the Materia Medica, illustrative extracts wherever the subject seemed to demand this course, and thus put it in the power of the student to realize the plan proposed by Dr Good.

Soon after the publication of the Treatise on Materia Medica, Dr Cullen received from various eminent and learned persons, scientific and professional, letters expressing their sense of the value of the Treatise as a work of utility to the physician. Among these we find Sir Joseph Banks, Sir Charles

Blagden, Dr Lettsom, John Andrew Murray, the learned Professor of Botany at Gottingen,—all eager to express their opinion of the merits of the work. Such productions are in many instances to be viewed as the mere effusions of politeness and friendly partiality. But in the present case, the subsequent course of events, some of which I have attempted to trace, showed that these persons simply anticipated that verdict, which the experience of at least thirty or forty years has only tended to confirm.

It comes not within the plan of these observations to trace the progress of Therapeutics and Materia Medica to the present time. My purpose is accomplished when I show, or attempt to show, the influence of the teaching and writing of William Cullen upon the teaching and writing of the nineteenth century. It is not easy to say where this influence ceases to show its effects. We have seen that it is distinctly perceptible during at least the first third of the century; and it is scarcely possible to conceive that any well-prepared writer should attempt at present even to treat of Therapeutics and Pharmacology without having previously given a reasonable amount of attention to the facts and doctrines, the reasonings and deductions, contained in the work of Cullen. But it would lead to unprofitable detail and unseasonable prolixity, perhaps exaggeration, were we to extend our disquisitions to all the works that have appeared on these subjects to the present time.

During the years in which Dr Cullen was occupied

in preparing the *Treatise on Materia Medica* for publication, not much occurred to diversify his life and require especial notice in these pages, beyond those events that have been already recorded. The only circumstances not hitherto mentioned were two examples of that high estimation in which the character of Dr Cullen was held as a scientific physician and an intelligent writer and teacher, and which, from the time at which they took place, could scarcely with propriety have been previously introduced. At Dublin there had been established, under the authority of the Medical Professors of Trinity College, a Society for the advancement of the Science and Art of Medicine in Ireland; and on the recommendation of these professors, who, it is said, had studied under Cullen, and held his name in much veneration, his name was, toward the close of 1787, enrolled among the list of honorary members of this Society. The intelligence of this proceeding was communicated in a letter of the 31st December 1787, from Dr James Cleghorn, the Professor of Anatomy, along with the official letter of the Secretary of the Society.\*

Of a more elevated and select character was the next testimony that was given to the scientific reputation of Dr Cullen. Peter Camper, the industrious and well-informed anatomist, who had been Professor of Anatomy and Surgery at Amsterdam, and who still held the title of honorary Professor,

\* This Dr James Cleghorn was the nephew of Dr George Cleghorn, the author of the instructive work on the Diseases of Minorca.



had breathed his last, at his house near Franeker, on the 7th day of April 1789, in consequence of an attack of pleurisy, at the age of fifty-seven years. The vacancy, which was thus occasioned in the Academy of Sciences at Goettingen, was speedily filled by electing Dr Cullen in the place of Camper. The announcement of this honour was communicated to him through the medium of Dr Girtanner, by Abraham G. Heyne, the learned editor of Homer, Pindar, and Virgil.

It appears, from some of the correspondence still extant, that about, or soon after the time at which the Treatise on Materia Medica was published, Dr Cullen was directing his mind to the question of publishing, as a fifth volume of the First Lines, a short view of the Diseases proper to Women and Children; understanding by this designation those Disorders which had not been very fully considered in the First Lines. A note from Charles Elliot the bookseller, dated 3d April 1789, shows that this gentleman had been considering the proposition, as he states, maturely, but seemed not perfectly willing to undertake the work on the terms proposed. Of this proposition no more is said. But while the mere proposition indicates the activity and energy of the mind of Cullen, it is not improbable that the members of his family, to whom the appearance of Dr Cullen must have at this time suggested some doubts as to the propriety of his undertaking any serious literary labour, had either directly dissuaded him from continuing his exertions, or at least had given him little encouragement to persevere in labours not

quite suited to his advanced period of life. It is indeed stated, in the volume of the Medical Commentaries for 1789, that Dr Cullen was proceeding with this fifth volume of the First Lines with his wonted zeal and activity. But this statement is manifestly one of those general effusions of politeness, indicating principally the good nature of the editor, and his high admiration of the talents and character of Dr Cullen,—not founded on any exact knowledge of the actual facts, and certainly without that intimate acquaintance with the condition and physical and mental capabilities of the individual, which is accessible to those who are living under the same roof, and in frequent intercourse with him. Dr Cullen was now in his seventy-ninth year. His life had been one of incessant professional and literary labour and industry; and though he could in no just sense of the term be said to be unfortunate, yet he had encountered a considerable amount of those unfavourable, if not positively adverse, circumstances which tend to exert an injurious influence both upon the physical and the moral part of the human constitution; which impair mental energy, embitter life and shorten its duration.

It is certain that the conduct of Dr John Brown caused him a considerable amount of uneasiness and vexation; and though he never mentioned the name of that person but upon one occasion, yet several of his friends were aware of the deep, severe, and hurtful influence, which the proceedings of Brown had produced upon his mind and feelings. It was not in this instance the mere feeling or consciousness of

ingratitude, the return of evil for great good and kindness, which wounded the mind of Dr Cullen. It was the apprehension that he was to be deprived of the credit, whatever that might be, which he had justly earned, by a life-long application to subjects of great difficulty and obscurity,—subjects in which the most competent judges had allowed that he had introduced method, order, precision, and elucidation, where previously were confusion, perplexity, and uncertainty; that this species of robbery was perpetrated by one who had enjoyed, in no ordinary degree, means of obtaining his confidence, and to whom Dr Cullen had been the means of communicating that kind and amount of knowledge which qualified him for acting as a teacher of some authority. A painful and humbling course it always is for a mind of elevation and independence to feel, that its possessor is placed in the position of proving claims, which had never before been called in question; to perceive that the intellectual property which it had cost him much industry and thought to acquire is ready to be carried off by the insidious attempts of a bold and unscrupulous pretender; and many with these feelings rather continue silent, than stoop to contend with an antagonist void of truth and without candour, and wait with patience in the confident hope that truth will at length, and after many struggles, prevail, and that justice may be done them during their life.

Part of this only, not the whole, was the fate of Cullen. He evidently felt severely the humiliating position of having his principles attacked and mis-

represented by Dr Brown, and after being distorted, appropriated by that person. But he seems to have thought also, that while it was impossible for him to descend into the field of controversy with one whom he had instructed, little expectation could be entertained, that during the period of his own life the profession could be made to understand how grossly they were deceived, and in how great a degree Cullen was wronged and misrepresented. In some sense Cullen probably attached more importance to this opposition and insidious misrepresentation than it deserved. It has been already shown that in this country the reputation of Brown was evanescent and transitory. But to Cullen, who could not positively foresee this result, the poisoned arrow came with little or none of the antidote. He felt also that few were competent judges of the matters in dispute; and in this belief he appears, while his corporeal frame was becoming feeble, to have resigned himself to what seemed unavoidable evil.

Another circumstance, which doubtless gave the mind of Dr Cullen not a little vexation, was his disappointment in the attempts that he made to procure the professorship, first of Natural History, and afterwards that of Botany, for his son Henry Cullen. It seems clear, from the letters of Cullen, that he thought that his son was entitled to obtain one or other of these offices. In both he was disappointed; and it may be inferred that these disappointments were not without serious effect upon the mind of one who had attained his seventy-ninth year.

Other matters, which it is unnecessary to men-

tion, and especially the knowledge of the expensive habits of a large family, tended all in the same direction to produce similar effects ; and when we know and daily see, how much evil is produced by those circumstances which act on the moral part of the constitution, it cannot be matter of wonder that Dr Cullen, approaching to eighty years of age, was not exempt from the operation of the usual causes of mental disquietude and depression.

In the meantime, his bookseller, Charles Elliot, died ; and this event, with the circumstances already mentioned, seems to have completely put aside all idea of publishing the proposed fifth volume of the *First Lines*.

Cullen continued, nevertheless, his several occupations. But that which appeared at this time, from April to October 1789, mostly to occupy his attention, was the regular, daily examination of letters of consultation from distant places, the preparation of answers often of considerable length, and entering into details as to the cases submitted to his judgment, and the kind of treatment required for each. All these letters he continued to write, so far as can be discovered from the copies, in his own handwriting ; and even from these copies, in their present state, it is impossible to resist the conclusion, that Cullen was a person of most remarkable activity of mind, great clearness of judgment, and the most extraordinary diligence and industry. The mere mechanical labour of dictating those letters to an amanuensis, or writing them, must have occupied a large amount of time. In general, they are ad-

dressed to the medical attendants of the patients ; in some instances, to patients themselves ; and, in some, to relatives or friends of the patients. In the months of March and April, however, it must be mentioned, we find a few letters signed by Cullen's son, Henry Cullen, a circumstance which acquires importance from what we shall afterwards have occasion to mention at a later period of this narrative. It is true, that Dr Cullen did not entirely at this time cease to write answers to the Consultation Letters, but persevered during the whole summer and autumn months in performing this duty, and for part of the winter months of November and December 1789. In a letter of the 30th May 1789, to Dr Robertson, he assigns as a reason for not sending an immediate answer to that gentleman, the circumstance, that he had been prevented by much College, that is, Academical, business.

It was nevertheless evident, that his health and vigour, both corporeal and mental, were giving manifest indications of failure ; and on this point we have his own testimony soon after the middle of October 1789. Dr William Pitcairn of London, a former pupil and friend of Dr Cullen, had, on the 1st of October 1789, given a note of introduction in favour of Dr Richard Warren, a London physician of deserved celebrity, and Fellow of the London College, who was desirous to visit Edinburgh, and see such persons and objects as might deserve attention. It was not at that time so common for English visitors to come to Scotland as it has been since the commencement of the nineteenth century. But

Dr Warren came to Scotland with Mrs Warren, his daughters, and son, and, among other celebrities, saw Dr Cullen, and received from him great attention, and every mark of kindness, which it was in the power of Cullen to bestow. Dr Warren and his family were much gratified by their visit to Edinburgh, and especially by the kindness of Cullen. But he seems to have been rather painfully impressed by the debilitated and frail appearance of Dr Cullen, and to have, in his anxiety, ascribed this to the abstemious mode of living, which he saw was observed by Cullen. In this apprehension, Dr Warren, on his way homeward, had no sooner obtained a little leisure at Newcastle-upon-Tyne, than he wrote to Dr Cullen on the 18th of October a sort of letter of remonstrance, in which he recommended him to take wine daily, as he understood he had previously done.\*

To this letter Dr Cullen did not write an immediate answer, for the reason, that he thought it most eligible to defer writing, until he was assured that Dr Warren was in London. In the letter of reply,† after returning thanks to Dr Warren for the interest which he showed in the health of Dr Cullen, and mentioning shortly his habits as to the use of wine, he informs Dr Warren, that “The infirmities

\* Dr Richard Warren communicated to the College of Physicians two papers; one on the Bronchial Polypus, or Albuminous Exudation of the Bronchi; read 11th August 1767; Volume I., Art. XVI., p. 407: the other on *Colica Pictonum*; read 4th August 1768; Volume II., Art. VII., p. 68. London, 1772. Dr Warren died in 1797.

† Dated October 1789.

of age have been gradually coming on for years past, and I do not find that they advance faster than in proportion to my years. They diminish the activity both of my body and mind ; but, as I judge, more in the former than in the latter. If you are of opinion that my drinking more wine would obviate the progress, I shall certainly attend to it, though I do not find that an additional glass of wine does me any service."

The truth, however, was, that the health and strength of Dr Cullen were in such a state of decay, that neither refraining from the use of wine nor the continuance of its use could have much effect in retarding or accelerating the approach of that change, which all circumstances showed might take place at no remote period. Within a few days after writing the letter to Dr Warren, that is, about the end of October, he entered on the course of Lectures on the Practice of Physic for the session 1789-90. This course it was destined that he should not finish. The marks of weakness and frailty were indeed too apparent, to permit any doubt as to the state of Dr Cullen to exist in the minds of those who beheld him. He had continued his exertions, nevertheless, during the month of November ; and it appears that he persevered in answering consultation letters during the whole of November and the first week of December 1789. From the fact of a letter of 9th December having been left unanswered for four weeks, when a reply was written by Henry Cullen on the 11th January 1790, it may be inferred, that he was at the former date little capable of attending to business



with his wonted vigour and regularity. The last letter written and signed by William Cullen himself is on the case of William Charteris, Esquire, on the 26th December. But he was now so feeble, and the symptoms had become so serious, that he was constantly confined to bed. On the 30th of December 1789, he sent to the patrons a letter resigning the office of Professor, as he felt that he was no longer able to perform the duties; and on the same day the patrons re-elected him, and appointed Dr James Gregory to be his assistant and successor.\* The ceremony of re-election must have been understood to be a mere matter of form,—a sort of acknowledgment of the great merit and beneficial services of the retiring professor. His symptoms are stated on the 9th of January 1790 in general terms to be alarming, requiring constant attention from his son and other children.

When the fact of the resignation of Dr Cullen was made known, it called forth from various public bodies a strong, spontaneous, and unequivocal expression of regret at what was an unavoidable evil, mingled, however, with sentiments of the highest esteem and admiration for the character of the individual, whose learning and talents, whose genius and long-continued exertions had done so much to elevate and adorn the Medical School of the University of Edinburgh, and to confer upon the Scottish metropolis a distinguished name and brilliant reputation in the history of medical science. First

\* Medical Commentaries, Volume XV. p. 499, for 1791.

came the patrons,—namely, the Lord Provost, Magistrates, and Town-Council, who, in the name of the community over which they presided, presented him with a valuable and elegant piece of silver plate, on which was engraved an inscription indicating their veneration and esteem for the man, and their grateful acknowledgment of his services as a physician and a public teacher.

Addresses expressing the high sense which they entertained of the abilities and services of Dr Cullen were sent to him by the *Senatus Academicus* of the University of Edinburgh, by the Royal Medical Society, the Royal Physical Society, and other bodies whom it is unnecessary to mention.\* One testimony, which was the spontaneous tribute of several of the most distinguished members of the profession, is, as a memorial, too important to be omitted. A general meeting of the pupils and admirers of Dr Cullen, resident in Edinburgh, was held on the twelfth day of January 1790 in the hall of the Royal Medical Society in Surgeon Square, to consider the most eligible means of erecting some permanent memorial of respect for his genius in the University Buildings, then in the course of erection. It was unanimously resolved, that a subscription for this purpose be imme-

\* These addresses were sent at various dates after the 1st January 1790. That of a society, named the American Physical Society, is dated the American Physical Society's Hall, Surgeon Square, 13th January 1790. The address of this body, which is supposed to be drawn up with considerable taste, and as proceeding from gentlemen at that time not British subjects, must be allowed to be impartial, is given in Dr Duncan's *Medical Commentaries*, Volume XV., p. 494, for 1791.

diately commenced; and as a committee for receiving subscriptions and superintending the execution of the work, the four annual Presidents of the Society for the time being, Drs Joseph Black, James Gregory, James Hamilton, Andrew Duncan, and Charles Stuart, from the College of Physicians, and Messieurs Alexander Wood and Benjamin Bell from the College of Surgeons, were appointed. The memorial was a marble bust, which is now placed in the large upper room or gallery of the College Library.

Dr Cullen continued, after his resignation, to linger in a state of great and gradually increasing weakness throughout the month of January, and departed this life at his house in the Mint on the Fifth day of February 1790, aged seventy-nine years and eleven months. In compliance, as we have understood, with the wish of Dr Cullen, his remains were conveyed to Kirknewton, the parish in which was situate his property of Ormiston Hill; and the funeral, which took place on Wednesday the 10th of February, 1790, was private. At the east end of the village, about half a mile eastward from the parish church, and three-quarters of a mile east from Ormiston Hill, is situate the burying-ground of the parish of Kirknewton, almost the lowest ground in that district; and nearly in the centre of this burying-ground is a quadrangular enclosure contained within four walls, bearing evident marks of age, without roof; and with a gateway provided with an iron gate on the west.\* In this spot, remarkable only for its peaceful and retired situation, remote

\* This enclosure is part of the old parish Church of Kirknewton.

from the noise and bustle of towns, and not much disturbed even by the sounds of agricultural industry, were deposited the remains of William Cullen, the most judicious and intelligent physician of his time in Scotland. No evident memorial or inscription distinguishes this sepulchre from those by which it is surrounded. But in the inner or western surface of the eastern wall there is inserted a marble slab, which records the name, the death, and the accomplishments of Cullen's eldest son, Robert, the judge, who died in 1810. By this alone the place is recognised as the tomb of William Cullen.

On the exact nature of the disease which terminated the life of Cullen, no authentic information has been preserved. Soon after the event of the death of Cullen was known, Vicq D'Azyr, who proposed to write an *Eloge*, or Biographical Notice, to be read before the Academy of Sciences, wrote to Dr Henry Cullen for information upon this and various other pertinent questions; but among the notices of this kind published in the writings of Vicq D'Azyr, no biography of Cullen is found. It is probable that the death of Henry Cullen, which took place eight months (11th October 1790) after that of his father, rather unexpectedly, had prevented him from forwarding to Vicq D'Azyr the information which the French Academician required.

Dr Cullen had himself, in 1784, expressed the opinion that he might suffer an attack of palsy. It may be regarded as an instance of the difficulty of forming correct opinions on a subject of this kind, that he never had any attack of palsy properly so

called, but presented only that general feebleness and frailty, which is the most common and certain accompaniment of old age.

Of the personal appearance and figure of Dr Cullen, the following sketch has been given by Dr Benjamin Rush from repeated observation.

“ Cullen was tall, slender, and had a stoop in his shoulders. His face was long ; his under lip protruded a little beyond the upper ; his nose was large and inclined to a point downwards. His eye, which was of a blue colour, was penetrating but soft ; and over his whole face was diffused an air of mildness and thought, which was strongly characteristic of the constant temper and operations of his mind.”

This description is not sufficiently minute and discriminating in details to enable readers to form any clear or distinct idea of the man. But between the bust already mentioned, a portrait taken by Martin in 1777, preserved in the hall of the Royal Medical Society, and another portrait by Cochran in the Hunterian Museum, Glasgow, it may be possible to frame some conception of the countenance, the look, and the general expression of the person, who exercised so remarkable an influence on the state of medicine in the eighteenth century. It must at the same time be remarked, that the description by Rush, and the features presented by the bust, equally belong to the last period of the life of Cullen, when he was bowed down by age ; frail, and approaching to the close of existence. The protruding and dependent aspect of the lower lip must be regarded as a slight paralytic relaxation, which did not belong to

the middle period of life ; and the stoop of the shoulders is equally an indication of the concluding period, when the muscles lose their energy, and mechanical forces prevail over vital organs. The portrait was taken at an earlier period, 1777, when Cullen was in the sixty-seventh year of his age,\* and communicates a more just idea of what Cullen, not in his best days, but before he had been subdued by age, must have been. It has been stated to the writer of these remarks, by two grand-nephews of Dr Cullen, that this portrait was regarded by such of the relations of the family as they had known, to present a likeness as exact as could be expected.

But the most successful efforts of the painter and sculptor, with whatever accuracy they may record exterior physical characters and physiognomical features, are seldom serviceable in conveying just impressions of the intellectual and moral character of the individual. The circumstances which can alone bring out these characters in prominent and visible relief, are sometimes overlooked and forgotten, often difficult to be understood, are not always easily communicated, and are sometimes misapplied in interpretation. In a large proportion of cases all minute circumstances of this nature are buried in oblivion ; and while the memorials of the talents, the attainments, and the mental powers are left, nothing is preserved to show, how their possessor differed from other men, in which of the intellectual and moral faculties he was most remarkable, and by what

\* Life of William Cullen, Volume I., pp. 458-9.

means he was enabled to rise so far above his predecessors and contemporaries. It is not always that autobiography supplies materials for this species of delineation ; and even when it does, how often does it mislead and perplex by frivolous, irrelevant, and unprofitable details ?

An object of interest, nevertheless, it always must be to illustrate the habits, disposition, and character of those who have been eminent in science, in literature, or in art, by the detail of such facts as disclose and throw light on the private moral nature of the individual. But for this purpose few materials are left in the case of Cullen, excepting those which relate to his professional labours and published writings, the letters of consultation which he left behind him, and those letters to pupils and contemporaneous physicians, several of which were published in the first volume of the present work. Some few facts Dr Thomson endeavoured to recover from the recollections of Mr Paul, the last amanuensis of Dr Cullen ; and though from the circumstances under which they were obtained, these are necessarily meagre and scanty, and relate chiefly to the latter period of his life, we shall study to present those which are most characteristic and important.

The life of Dr Cullen was one of great if not incessant activity and labour. In the latter period of his life, when he had possession of Ormiston Hill, he devoted much attention to the subject of agriculture and arboriculture, which he had there opportunities of studying practically. When in Edinburgh, he rose about a quarter before seven o'clock, and till nine was

occupied in dictating to an amanuensis the materials for the *Materia Medica*. At ten o'clock Dr Cullen went out and visited patients in the town or neighbourhood. In these days this duty was mostly performed in a sedan chair, in order to get through the closes and wynds, or narrow steep lanes, in which the dwellings of the inhabitants were then situate. He usually returned about twelve o'clock, or soon after that hour; and sometimes he remained at home the whole day afterwards, occupied in reading consultation letters and writing answers, or in seeing those patients who came to obtain his advice.

In his professional visits he was very punctual to the hour appointed; and when he had consultations with other physicians or with surgeons, notes were sent to them before eight in the morning, fixing the respective hours at which he was to meet them.

In the evening he sometimes made one of a party at whist; but this was the only relaxation or amusement in which he indulged when resident in Edinburgh.

It is unnecessary to say, that in the performance of his academical duties Cullen was regular, systematic, and conscientious. All the dissertations which were committed to his examination he read with attention, and marked carefully such passages as seemed to him to deserve notice, inquiry, comment, and particular commendation.

The mode of lecturing observed by Cullen was that of a man perfectly acquainted with his subject. There is no proof left to show, and no reason to believe, that in any of the courses which he delivered,



the lectures were fully and literally written. When he taught the Institutions of Medicine, it was only in the year 1772 that he published the Institutions of Medicine, or Outlines of Physiology, to show the pupils the several divisions of the subject, and the order in which he was to treat them. But when he came to lecture, he amplified and illustrated, in different degrees and in different modes, all the subjects treated. This he was easily able to do, at once from the accurate knowledge which he possessed of physiology, as it then stood, and from that accurate personal observation and ingenious reflection and induction, in which he had few equals. When he came in 1773 to lecture solely and exclusively on the Practice of Physic, or Special Pathology, Nosology, and Therapeutics, his observation and experience as Clinical Teacher rendered him still more completely master of the subjects to be treated; and while the First Lines served as the basis of his instructions, the comments and statements by which he illustrated and amplified these short, and, in several points, aphoristical elements, formed by far the largest, and certainly not the least attractive, portion of his prelections. We are further informed, upon sufficiently credible authority, that during the latter years of his life he entered the lecture-room with only short notes of the subjects to be discussed and illustrated, and that he trusted altogether to his correct knowledge, his power of expression, and extemporaneous elocution, for conveying, in a distinct and impressive form, his instructions to his pupils. In short, though the sub-

stantial general matter of the Lectures was preserved in a state not greatly changed in the First Lines, yet he was incessantly altering them from year to year, enlarging in one direction, abridging and curtailing in another, and modifying everywhere.

It is possible, nevertheless, that certain portions of the Lectures on particular subjects may have been written fully and verbally. The Introductory Discourse on the History of Medicine, which is published by Dr Thomson in his edition of the Works, is more ample in information, more complete in methodical division, and more minute in details, than the Sketch which Cullen prefixed to his own edition of the First Lines. The difference between these two Introductions may serve to communicate some idea, though not a perfect one, of the manner in which Dr Cullen enlarged his lectures in the course of delivery.

In the act of lecturing, his language was well chosen, clear, and simple, his manner impressive and interesting, his delivery easy, fluent, and agreeable. "From the moment that he ascended his chair," says Dr Rush, "he commanded the most respectful attention from his auditors ; insomuch, that I never saw one of them discover a sign of impatience during the time of any of his lectures." He always studied carefully the previous evening the lecture to be delivered on the following day.

He appears to have taken considerable pains, in preparing his Treatise on Aliments, to obtain accurate information on the effects of Diet. For this purpose he procured a statical chair, and subjected his

amanuensis to various experimental trials with different kinds of food. But these trials seem not to have led to any very decided results.

During his whole professional life Dr Cullen continued to reside in the same house in the Mint; and in that house he died. This house, for it is still in existence, was then a sufficiently respectable abode, according to the ideas and the practice of the inhabitants of Edinburgh, at the time when Cullen lived. The greater part of the members of the professions of law and medicine occupied houses of similar appearance, and situate in similar localities, especially the wynds, closes, or narrow lanes passing from the High Street, the Lawn Market, the Canongate, and the Cowgate; and even the supreme judges of the courts continued to a late period in the eighteenth century to occupy houses in these situations. During the latter part of his life, the houses in Argyle Square and Brown Square began to be erected, and at a period rather later, those in George Square. It was then only that professional persons began to occupy houses larger, more commodious, and placed in better situations. But Dr Cullen, like many other persons, continued to occupy the old house in the Mint, which, it must be remembered, was near the scene of his academical duties, and was, considering the time, sufficiently convenient for visiting patients and being visited by them.

In the latter part of his life, subsequently to the year 1778, when he became possessed of Ormiston Hill, he spent much of his time in that place, where his occupation in planting, improving, and in various

agricultural pursuits, enabled him at once to gratify his taste for the practice of agriculture, and furnished a form of recreation, which rendered him more capable of pursuing his professional studies and academical duties in a vigorous and satisfactory manner. Everything in his history, and many notices in his letters, show that to this place Dr Cullen was strongly attached, and that in its improvement he took great interest. But this was only the revival of a passion which, in the breast of Cullen, had been dormant, not extinguished. The love of the country, the pleasure of contemplating woodland scenes, cultivated fields, and thriving pastures, and beholding or joining in rural occupations, is a passion common to many orders of mankind. The merchant, the lawyer, the physician, the soldier, all look forward to the period when, released from the urgent pressure of active professional duties, they may in some quiet retreat spend the concluding scenes of life, undisturbed by the bustle and tumult of the meridian of their days.

“ *Saltem remoto des Pater angulo,  
Horas senectæ ducere liberas ;  
Tutumque vulgari tumultu  
Surripias, hominumque curis.*” \*

In these lines did Gray, when passing a day at a religious house in the Alps of Dauphiny, give utterance to a sentiment which is common to many, though not always expressed, and which, though often forgotten, is never entirely obliterated.

\* Works and Memoirs of Life of Thomas Gray, by W. Mason. York and London. 4to. 1775. P. 118.

With Cullen these feelings were innate, and acquired, from various circumstances, peculiar force as he became aged. He had spent his early days in the country, in a district where the natural beauties of the scenery could not fail to promote the growth of this sentiment ; and though the middle period of his life was passed in populous towns, where he had few opportunities of gratifying this taste, yet as he advanced in life, these sentiments revived with increased force, and led him to seek recreation and amusement in the country and in rural occupations.

It was not, however, as a speculative and intellectual admirer only of the beauties of rural scenery and the tranquillity of a country life, that Dr Cullen became the possessor of Ormiston Hill. He had, when Professor of Chemistry, delivered a course of Lectures on Agriculture, in which he brought forward not a few new ideas and doctrines on the nature of soils, the nutrition of plants, on manures and their mode of operation, and the adaptation of crops to suitable soils. He had never ceased to feel the desire to subject his theoretical doctrines to the test of actual trial ; to extend, to rectify, and to confirm his knowledge by observation and experience. This long cherished hope he thus expected to carry into effect ; and so far as difficulty was concerned, Ormiston Hill afforded ample opportunities.

Unless the reader bear this in remembrance, he will feel it difficult to understand, upon what principle or for what reason Cullen was so much attached to this spot, which, though not devoid of beauty, is not exactly the region upon which a farmer, who takes

pleasure in rich remunerative crops, would have chosen to expend money and industrial labour. But it was not distant from Edinburgh; and even its natural difficulties acted as a salutary excitement to the active mind of Cullen.

On the old Glasgow road, which runs by Midcalder and the Kirk of Shotts, about eight miles from Edinburgh, nearly opposite to Hatton, a parish road issues from the public turnpike road, leading first nearly southward and then almost due west. This road passes through the village of Kirknewton, and then ascending regularly as the ground rises, about three hundred yards beyond the parish church of Kirknewton, places the traveller at the base of Ormiston Hill. The common road is continued westward, and leads to Midcalder and various other places. At present it is crossed on the level by the Caledonian Railway, the Kirknewton station of which is on the north, close at hand. On the south rise the grounds of Ormiston Hill, with an exposure to the north; and a short distance up the hill, on the level ground at its summit, stands the old house which was occupied by Dr Cullen. The house is plain enough, altogether without architectural beauty or pretensions; and indeed in the eye of the æsthetical observer it must be pronounced to be an object sufficiently commonplace, presenting nothing attractive or imposing. It consists of two houses, very narrow, united by their gables, running nearly east and west, or, with a northern and southern exposure, forming a long, narrow, and unsightly building. The eastmost of these two houses was erected by Cullen himself.

The apartments of both are small, occupying the entire breadth of the buildings, which is about 21 feet. The ceilings are low ; the windows small. But, though these buildings are in the fashion of that day, when money was scarce, and before the taste for showy houses and magnificent edifices with spacious apartments had arisen in Scotland ; there is about the whole a pleasing air of comfort and commodiousness ; and the view over the grounds from the south front is diversified with an agreeable variety of hill and level country. In this unpretending abode and the surrounding grounds, Cullen spent a large proportion of his available leisure during the months of summer and autumn ; and we have been informed on authority, which may be regarded as good, that he frequently repaired to Ormiston Hill, to see how his plans for improving and embellishing the place prospered, and to think of others that might seem requisite and suitable.

The soil of Ormiston Hill is mostly a light gravel ; and as the ground rises considerably, and has a northern exposure, the climate is rather cold and ungenial. In the upper table-land heath still grows in abundance ; and moss is by no means a rarity. In the summer months the place looks beautiful ; the vegetable world is then sufficiently vigorous ; and the views from the grounds upon the fertile, well-wooded, and well-cultivated fields of the Lothians are magnificent. But in the beginning of spring, the close of autumn, and in winter, the climate cannot be inviting. Kaimes and Dalmahoy Hill, considerable elevations, and various other heights, outliers of the west-

ern portion of the Pentland range, are seen at no great distance. Upon these places the sun must shine with feeble influence and abated force during seven or eight months of the year; and it was not without reason, that, when Dr Cullen was showing to Alexander Wilson, professor of Astronomy at Glasgow, the beauties of Ormiston Hill, and explaining the success with which its proprietor cultivated various fine trees and delicate exotics, the latter gentleman dryly put the question, what was the average height of the barometer, and added his belief that it could not stand so high as at the sea-level by four-tenths of an inch.

Notwithstanding, however, its elevated situation and its northern exposure, Ormiston Hill was the delight of Dr Cullen; and between planting trees, clearing and rooting up heath, reclaiming and improving moss grounds, levelling, covering gravel with new soil, draining, and cultivating shrubs and flowers imported into the place, he succeeded in giving to it a degree of rural beauty and vegetable profusion and variety which struck every one who visited the territory. The relics of some of this industrious exertion may be at present traced in the adventitious Flora of Ormiston Hill.

Between two hundred and fifty and three hundred yards to the westward of the spot on which stands the house of Ormiston Hill, the ground begins to descend at first gradually, then by an oblique slope more directly, forming a shelving bank or declivity about thirty or forty feet downwards. This bank forms, with a similar one a little farther westward, and considerably more steep, a deep hollow or dingle, following, with several windings, a general



direction from south-west to north-east, the bottom of which is traversed by a small rivulet running in the same direction, and which a few miles further to the north-east becomes the principal source or main stream of Gogar Burn. The dingle thus formed is at this point the western boundary of Ormiston Hill; for west of this and the rivulet which traverses its hollow, the territory is that of Ormiston.

This dingle or ravine, though on a small scale, possesses many elements of natural beauty; and these Dr Cullen had laboured diligently to develop and improve. The site of the dingle, lying between two elevated banks, the clear stream murmuring at its base, and following the windings of the ravine, the silence and retirement seem strongly to have struck his imagination. He made a walk along the margin of the rivulet; he planted poplars, birches, firs, and other trees along the bank; he scattered numerous perennial plants over the whole space; and, lastly, he constructed a small alcove to afford rest and shelter when required. In this retreat, in which, during summer, the trees sufficiently excluded the rays of the sun to render it pleasant, the traditions of the neighbourhood say Cullen spent much of his time during the summer recess; and the physical characters of the place are unquestionably of that kind, that they might make him for the time forget, that he was so near Edinburgh, the scene of his professional labours, and the anxious cares of the physician's life.\*

In order to form a correct judgment of the intel-

\* Note Y.

lectual character of William Cullen, it is only requisite to consider his writings with attention and candour, and to bear in mind the state of medical science at the time, at which he began his career as a lecturer and writer. To the reader who has considered with any attention what has been stated in the course of these volumes, this will not be difficult, and must have come out clearly, though gradually, in the history already given.

The leading characters of the intellect of William Cullen were great energy, clearness of perception, accuracy in observing, soundness of judgment, logical precision in reasoning and deducing inferences, much originality in all his views, and a remarkable faculty of concentrating information on all points and making it illustrate the subject of medical pathology and therapeutics. Cullen could not be said to be an anatomist; and in physiology it must be admitted that he was inferior both to Haller and to Whytt. He had neither the profound and accurate knowledge of the structure of the human body, nor the intimate acquaintance with the history of facts, arguments, opinions, and doctrines possessed by the former; nor could he be compared with the latter in his habits of experimental investigation, and elaborate inquiry, and the faculty of acute and ingenious reasoning. In several qualifications, however, he was decidedly superior to each of these eminent physiologists; first, in the penetration and sagacity with which he could investigate and illustrate the psychological branches of physiological science; secondly, in his power of distinguishing and selecting, amidst an immense

mass of heterogeneous materials, those facts only which are regular, constant, and universal, and rejecting those which are accidental, frivolous, or partial, or imperfectly ascertained; thirdly, in his faculty of precise distinction, accurate definition, and methodical arrangement; and, lastly, in the readiness with which he could apply the results obtained by his predecessors, contemporaries, and pupils, to the rectification, simplification, and general improvement of medical theory and practice.

Such, so far as impartial and attentive consideration enables us to form a judgment, were the leading characters in the mind of Cullen, by which he was enabled, from a situation comparatively obscure, to rise to a position of great eminence in the profession of medicine, and to exercise upon its theory and practice a powerful and salutary influence. Other circumstances, indeed, there were of an accessory character, which at once contributed to favour, and in some respects tended to oppose the efforts of Dr Cullen.

When we contemplate that portion of the eighteenth century which corresponds with the active period of the life of this physician, extending from 1746 to 1789, and contrast the beginning of this period with its close, the great difference is perceived in a clear and unambiguous light. It is unnecessary to repeat what has been already said in various parts of these volumes, especially when speaking of the state of *Materia Medica*, on the condition of medicine in the first half of this century. When Cullen entered on the practice of medicine in the year 1736, John Freind, one of the most emi-

ment physicians of London, had been some years dead, and Richard Mead and Sir Hans Sloane were the leading medical authorities. Freind and Mead, though differing in age, were united by strong ties of friendship; both men of great talents, highly cultivated by varied learning and philosophy. Yet in attempting to make so moderate a reform as that of recommending the employment of aperient medicine in the treatment of small-pox and its secondary fever, both encountered the most determined opposition.

The success of Newton in applying geometry to explain the principles of natural philosophy had persuaded many learned physicians, that in medical theory also geometrical reasoning was quite indispensable; and accordingly we find that the language of Freind and Mead is upon this point equally strong and decided. Both of these physicians, indeed, were warmly attached to the principles of the mechanical philosophy and the mathematical system of physiology,—a course in which they were preceded and encouraged by Pitcairn and George Cheyne, for the talents of both of whom Mead expresses the highest admiration. Towards the close of his life, however, Mead got a glimpse of the inapplicability of this theory, and became somewhat inclined to the principles of Stahl; a change which is generally ascribed to the influence of Dr Frank Nichols over the mind of Mead. The Essays, indeed, of James Keill and Jurine, while they showed what might be achieved by the application of mechanical principles and mathematical reasonings to physiology, indicated also what they were unable to accomplish. Nor

were the labours of Colin Maclaurin without effect ; which, by explaining the true and legitimate application of the researches of Newton, ought to have satisfied all sound thinkers, that medicine was not the field for the application of geometry ; and that, however useful when applied to mechanics and physics, the application of mathematical reasoning to elucidate the actions of animal bodies is barren and unprofitable.

While medical theory was thus shaken in all its parts, there gradually arose, about the middle of the century, a new system, which, by dissevering all connection with the science of abstract quantity, and allowing medicine to rest on observation and experience alone, in the hands of Huxham (1738–48), George Cleghorn (1751), Sir John Pringle (1752), Donald Monro (1764), Francis Home (1759), Brocklesby (1764), the two Linds (1763–68), and Sims (1773), threw into the shade all other means of acquiring medical knowledge and regulating medical practice. It was during the early part of this period (1746–48) that Cullen began to observe, to reason, to teach ; during the entire course of this period he was diligently occupied in reducing into a general system the results presented by these and other practical observers, in confirming, rectifying, and modifying them by his own experience and that of predecessors and contemporaries ; and it must be admitted, that for these purposes he had fallen upon good days.

At no time, certainly, had this country beheld together so many able men, as appeared during this portion of the eighteenth century. Then was seen the acute penetrating mind of David Hume, equally

distinguished for speculative disquisition and sagacious practical views ; who, though his skeptical turn led him into various deviations, yet at once, by rigorous logical reasoning, by judicious selection and arrangement of known facts, by new views and by his errors, rendered to philosophical inquiry more service than had been done by any single individual ; who called into activity the patient analytic mind of Reid, the profound and philosophical Campbell, and the elegant genius of Gerard. Then appeared Adam Smith, equally distinguished by his accurate knowledge of philosophy, ancient and modern, his talent for ethical disquisition, the soundness and comprehensiveness of his views in political economy, and his faculty of clear, elegant and harmonious writing. Then also appeared Henry Home, afterwards Lord Kaimes, a person who distinguished himself as much by his attainments in polite literature, as in his own professional studies. During the same period lived Robertson, Ferguson, and Henry ; of the two former of whom it is difficult to say whether they were more excellent as historians or as philosophical inquirers. With all these men Cullen was more or less intimate ; with some, as David Hume, Lord Kaimes, Smith, and Robertson, on the most intimate terms ; and it is impossible to doubt, that while his friendship was one effect of the high estimate, which they had formed of his talents and acquirements, the society of such men was calculated to exercise upon the mind of Cullen an influence most favourable to the development and cultivation of his own mental faculties. It is in this spirit that

we find the benevolent disposition of David Hume looking upon his contemporaries with sentiments of pleasure and gratification, and after all his changes and wanderings, reverting with untravelled feelings to the select and concentrated literary circle of Edinburgh. In this circle Cullen lived as professor and scientific physician ; and while he was everywhere known in the latter character, he was not less remarkable as a member of that assemblage of accomplished persons, by whom, during the eighteenth century, the small capital of Scotland became the centre of whatever was ornamental and useful in literature and in science.

Those readers who have perused with any degree of attention the pages of these volumes, will have little difficulty in perceiving, that Dr Cullen, in making the various changes in the Theory and Practice of Medicine, which it has been our duty to record, was neither rash, reckless, indiscriminate, nor injudicious. He cannot be classed with those innovators who regard as useless and untrustworthy all that has been done before their own time ; who refuse to their predecessors and contemporaries all credence ; who, in desiring to remodel the science and reform art, think it necessary first to destroy and demolish all that had been previously established. Assertions of this kind were indeed propagated regarding his instructions ; but they were the offspring of imperfect knowledge or of misrepresentation. Cullen proceeded in a manner much more rational and cautious. The main object which he had in view was to separate truth from error, facts

from fancy, theory from hypothesis, and serviceable practical instruction from the illusions with which a long course of ignorance, indolence, error, and superstitious veneration for authority had involved the Theory and impaired the efficiency of the Practice of Medicine. In the performance of this duty, he treated with reverence and delicacy all the truly useful parts of Medical Doctrine and Practice. The strong and powerful judgment with which by nature he had been endowed, he industriously employed in distinguishing what was sound and good from that which was worthless and unprofitable. The latter only he rejected; the former he retained.

A prominent feature in the life of Dr Cullen was great generosity, and liberality, which was habitual, and, as it were, inseparable from his nature. Regulated by financial considerations, and kept in due relation to his means, this would not have been injurious. But it must be allowed that even this virtue, carried to extreme, was in his case productive of effects which, after his death, caused a sufficient amount of evil. Dr Cullen had been throughout life indifferent to pecuniary matters; and although for several of the concluding years of his life his revenues must have been, for these days, considerable, it was found, after his death, that nothing was saved. The consequence of this was, that not long after his death it became necessary to dispose of Ormiston Hill, which thus in 1792 passed into other hands.

It is a remarkable fact, that of Dr Cullen's large family of seven sons and four daughters, very few



lineal descendants are left. The eldest son, Robert Cullen, known as Lord Cullen, died without family at Edinburgh on the twenty-eighth day of November one thousand eight hundred and ten, in the sixty-eighth year of his age.\*

It has been already stated in a former part of this volume, that Dr Henry Cullen died on the eleventh day of October 1790, when he must have been about thirty-one or thirty-two years of age.

Archibald Cullen, who, after having studied Medicine and graduated at Edinburgh in 1780, went to the English Chancery Bar, married, in 1782, Fennella Sinclair, the only daughter of Charles Sinclair, Esquire of Alrigg, by Elizabeth Sutherland, sister to James Sutherland, to whom was restored in 1826 the title of Lord Duffus. By this marriage Archibald Cullen had three sons; William Cullen, Sinclair Cullen, and David Cullen; and two daughters, Anna and Robinia Marion Cullen.

Of the sons, the eldest, William Cullen, went in 1804 to India as a cadet in the Artillery of the Madras Presidency. In this service this gentleman has risen to the rank of Lieutenant-General; and he further holds the office of Political Resident at Travancore. Lieutenant-General William Cullen, when we last received intelligence of him, (1859), was in the seventy-fourth year of his age, and is at this time, being grandson, the nearest lineal descendant of Dr Cullen.

Sinclair Cullen, the second son of Archibald Cul-

\* Robert Cullen was born on the twenty-second day of September one thousand seven hundred and forty-two.

len, devoted his attention to the profession of law in England, but died in 1829, before he had much opportunity of making himself known.

David Cullen, the youngest son of Archibald Cullen, entered the army, and was serving with honour in the Peninsular War, when he was killed at the siege of Burgos in Spain in 1812.

The elder daughter of Archibald Cullen, Anna, died in 1827. The younger daughter, Robinia Marion, became the wife of Mr John Loch, well known as a Director of the East India Company, a gentleman highly esteemed, and of great influence in that body. This lady, after giving birth to a daughter on the 28th December 1823, died early in January 1824; and the effect of this bereavement on the mind and health of Archibald Cullen, her father, was so severely felt, that in the course of a few months afterwards his remains were placed in the same tomb. Archibald Cullen is described by those who knew him as a man of talents of the highest order, and a mind well cultivated, but in manner reserved and shy. He was many years a Commissioner of Bankrupts; but resigned this duty upon being made King's Counsel. Mrs Archibald Cullen, who survived him a few years, died in 1830.

We have mentioned one lineal descendant of Dr Cullen in the person of Lieutenant-General William Cullen. There is yet another, though one step farther removed. It has been stated that Robinia Marion, the youngest daughter of Archibald Cullen, became the wife of Mr John Loch. From this marriage there were one son and one daughter. The

son entered the Navy, and died on the coast of Africa in the public service. The daughter, Marion Fenella Loch, became the wife of Edward Marjoribanks, junior, a partner in the banking-house of Messrs Coutts and Co., London. Mrs Marjoribanks, therefore, is great-granddaughter of Dr Cullen; and as of this family there are three sons and three daughters, these children may be regarded as the only lineal descendants, in the fourth degree, from Dr Cullen, at present in this country.

James Cullen and William Cullen, younger sons of Dr Cullen, went to India, and there died comparatively young.\* John Cullen also died in early life, though it is not stated where.

Another son of Dr Cullen, namely Charles Cullen, held an important situation in the London Custom-house. This gentleman died about 1827, at Camberwell, in Surrey, unmarried.

Of the daughters of Dr Cullen, Robina Cullen was married on the 27th September 1790 to John Millar, Esq., Advocate, a son of John Millar, Professor of Civil Law in the University of Glasgow. The two other daughters, Elizabeth and Margaret, died unmarried.†

To those who are acquainted with the series of periodical Essays published at Edinburgh between January 1779 and the end of December 1786, under

\* See Note Z.

† Mrs Millar died in 1844; Miss Margaret Cullen died about four or five years previously. Miss Elizabeth Cullen, the eldest daughter, was affected with great if not complete loss of sight during the latter years of life.

the names of the *Mirror* and *Lounger*, it is scarcely necessary to say, that Robert Cullen was one of that assemblage of writers who endeavoured, with the able assistance of Henry Mackenzie and William Craig, subsequently a judge in the Court of Session by the title of Lord Craig, to instruct and direct the taste of their countrymen, occasionally to amuse, sometimes to elevate the feelings by interesting narratives. But as these pages may be read by some, who are scarcely aware of the existence of those Essays, or know them only by name, it is proper to say that Robert Cullen wrote in the *Mirror* the thirteenth Essay, the twenty-seventh, the forty-eighth, and in the *Lounger*, the fifth Essay, the twelfth, and the seventy-third. These papers are written in an easy, natural style, and show on certain subjects knowledge and acuteness, observation, and reflection. In the first, in which the author directs attention to the beauties of the effusions published under the name of Ossian, he shows less discrimination than would in the present day be expected of a writer on that subject, in assuming, or at least not doubting, the authenticity of these celebrated compositions. But in this, it must be remembered, that Robert Cullen, then a young advocate, only followed the example of Dr Blair, Dr Henry, and other respectable authorities, all of whom more or less implicitly assumed the authenticity of these writings. David Hume was the only person in Scotland, who at that time had ventured to entertain and express doubts, and show by what evidence the question was to be tried; a duty which

was several years afterwards skilfully performed by Malcolm Laing.

Robert Cullen had also contemplated the scheme of writing a biographical account of his father ; and with this intention in view, when Dr Andrew Duncan, senior, applied to Mr Cullen for materials for writing a Life of Dr Cullen, he declined complying with the request of Dr Duncan. The proposed biography of Dr Cullen never appeared, whether from the pressure of professional duties or other engagements, is not known. But it may be doubted whether, with all the talents which it was admitted Lord Cullen possessed, he was master of that kind and amount of information, which could have qualified him for writing a useful history of the life, and giving just views of the character, of Dr Cullen.

It has been already mentioned, that Archibald Cullen, who went to the English Chancery Bar, wrote a work on the Bankrupt Law of England, of which authorities speak in favourable terms. Margaret Cullen was one of the earliest of our lady authors, and wrote two works of fiction, "Home" and "Mornton." Of the merits of these volumes it is unnecessary here to give any opinion.

## PUBLISHED WRITINGS OF DR CULLEN.

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It may be convenient to present at one view an authentic list of the published writings of Dr Cullen, following the chronological order in which they appeared.

I. On the Cold produced by Evaporating Fluids, and of some other means of producing Cold. By Dr William Cullen, Professor of Medicine in the University of Glasgow. [May 1, 1755.] Essays and Observations, Physical and Literary, Read before a Society in Edinburgh, and published by them. Volume II., Art. VII. p. 145. Edinburgh, 1756.]

This Tract was afterwards published in 1782, along with that containing the Experiments upon *Magnesia Alba*, Quick Lime, and other Alkaline substances, by Dr Joseph Black, which first appeared in the same volume of the Essays, Physical and Literary.

II. *Catalogus Materiæ Medicæ*. 8vo. Edinburgh, 1761.

This Catalogue, which Dr Cullen prepared for the use of the pupils attending the course of Lectures on *Materia Medica*, which he delivered in 1761, is, in an improved and amended form, republished in the *Treatise on MATERIA MEDICA* of 1789.

III. When Dr Cullen was inducted on the 1st November 1766 to the duties of the office of Professorship of the Institutions of Medicine, he had no text-book for that department of the science. With the view of supplying this defect, he pub-

lished, in 1767, an Edition of the *PRIMÆ LINEÆ* of Albert von Haller, from the Third Edition by the author.

ALBERTI V. HALLER *PRIMÆ LINEÆ PHYSIOLOGIÆ*, in usum Prælectionum Academicarum, ad Editionem Tertio auctam et emendatam D. 22 Nov. 1764, expressæ. (4 Kal. Nov. 1766.) Accedit Rerum Index. Edinburgi, 1767.

What is here called an Index is rather a minute, detailed, analytical Table of Contents, presenting an enumeration of all the subjects treated in the volume, and the divisions of each.

This is the best edition of that useful work.

IV. *Synopsis Nosologiæ Methodicæ*. 8vo. Edinburgi, 1769.

*Synopsis Nosologiæ Methodicæ*. 8vo. Edinburgi, 1772.

Several editions of the *Synopsis* were published by Dr Cullen himself, and by other editors after his death. The best, by himself, is the following.

*Synopsis Nosologiæ Methodicæ, Exhibens Clarissimorum Viro-  
rum, SAUVAGESII, LINNÆI, VOGELII, SAGARI, et MAC-  
BRIDII Systemata Nosologica*. Edidit, suumque Proprium Sys-  
tema Nosologicum Adjecit Gulielmus Cullen, M.D., et in Acad.  
Edinb. Med. Pract. Prof. et Medicus Regius apud Scotos  
Primarius, &c. &c. Editio Quarta, Emendata et Plurimum  
Aucta. Duobus Tomis. Edinburgi, 1784.

Editio Quinta. Edinburgi, 1792.

Editio Sexta, Emendata et plurimum Aucta. Duobus  
Tomis. Edinburgi, 1795.

After the death of Dr Cullen, various editions of the *Synopsis* were published, mostly of small size, to suit the convenience of students. One was published with the *Classification of Cutaneous Diseases* by Willan; and one was published by the late Edward Milligan, M.D., &c.

V. *Lectures on the Materia Medica*, as delivered in 1761, by William Cullen, M.D., Professor of Medicine in the University of Edinburgh, and now printed from a correct copy which has been compared with others by the editors. 4to. London, 1772, 1773.

This is the unauthorized or surreptitious edition of these Lectures.

During the years between 1767 and 1773, Dr Cullen was occupied in preparing, for the pupils of the Institutions of Medicine, a Text-Book in the English language. The first part of this Treatise, embracing Physiology, was published in 1773.

VI. Institutions of Medicine, Part I., comprehending Physiology of the Human Body. Edinburgh, 1773.

Institutions of Medicine. Part I., comprehending Physiology. Second Edition. Edinburgh, 1777.

Institutions of Medicine. Part I., Physiology. The Third Edition, corrected. Edinburgh, 1785.

VII. A Letter to Lord Cathcart, President of the BOARD OF POLICE in Scotland, concerning the Recovery of Persons Drowned and seemingly Dead. By William Cullen, M.D., First Physician to His Majesty, &c. &c. Edinburgh, 1776.

VIII. (1.) First Lines of the Practice of Physic. By William Cullen, M.D., &c. Volume First. Edinburgh, 1777. 8vo.

First Lines of the Practice of Physic. By William Cullen, M.D. Edinburgh. Volume Second. 1780. Volume Third, 1783.

First Lines of the Practice of Physic. By William Cullen, M.D. Volume Fourth. Edinburgh, 1784.

First Lines of the Practice of Physic. By William Cullen, M.D., &c. Now first completed. In Four Volumes 8vo. Edinburgh, 1785. Again in 1787.

Several editions of this work were afterwards published.

(2.) First Lines of the Practice of Physic. By William Cullen, M.D., late Professor of the Practice of Physic in the University of Edinburgh, &c. &c. A New Edition, carefully compared with the Author's most correct edition. In Two Volumes. Edinburgh, 1803. 8vo. Again in 1812. 8vo.

This edition is stated to have been carefully revised by Dr



James Gregory, the successor of Dr Cullen in the Chair of Practice of Physic.

(3.) An Edition of the First Lines was published by Dr Peter Reid.

(4.) The works of William Cullen, M.D., Professor of the Practice of Physic in the University of Edinburgh. Containing his Physiology, Nosology, and First Lines of the Practice of Physic; with numerous Extracts from his Manuscript Papers, and from his Treatise on the Materia Medica. Edited by John Thomson, M.D., F.R.S.S.L. et E., &c. &c. In Two Volumes, 8vo. Edinburgh, 1827.

(5.) First Lines of the Practice of Physic. By William Cullen, M.D., &c.

A New Edition, with an Appendix, containing a View of the most important Facts which have been ascertained, and Principles which have been adopted, in regard to the Nature and Treatment of Diseases since the death of the Author; commenced by the late William Cullen, M.D., &c. &c.; and continued and completed by James Craufurd Gregory, M.D., F.R.S.E., Fellow of the Royal College of Physicians, &c., and one of the Physicians to the Royal Infirmary, Edinburgh. In Two Volumes, 8vo. Edinburgh, 1829.

IX. A TREATISE of the MATERIA MEDICA. By William Cullen, M.D., Professor of the Practice of Physic in the University of Edinburgh; First Physician to His Majesty for Scotland; Fellow of the Royal College of Physicians of Edinburgh, &c. &c. &c. In Two Volumes, 4to. 1789.

Besides the works here enumerated, there were published at London, in the year 1797, seven years after the death of Dr Cullen, Clinical Lectures by Dr Cullen in 1765 and 1766. This volume, however, is one of questionable authenticity; and though it professes to give Clinical Lectures by Dr Cullen, it cannot justly be regarded as a production of his. It was merely a speculation of the bookseller; and, indeed, any book bearing the name of Cullen, for some years before his death, and after that event, would have been readily bought.

## APPENDIX.



## APPENDIX.

### NOTE A, p. 3.

IN the same Preface, Dr Walker states : “ During the short while I lived at Glencross, I went one season to the Goat Whey, in Breadalbane, along with Dr Cullen, when our whole time was employed in examining and collecting the fossils in that part of the Highlands.” And again, “ It is now sufficiently evinced that the numerous fossils on the globe cannot be investigated, discovered, and ascertained by either of these two methods (viz., the internal or the external characters), independently of the other. This leading principle was the maxim of the two best judges I have ever known, Dr Cullen and the Earl of Bute, whose opinions and instructions I always found judicious and useful.” And in a subsequent passage, Dr Walker says, “ In the following catalogue, fossils are distributed into Classes, Orders, Genera, Species, and Varieties. This fivefold division, though arbitrary, is excellent, and has now, from experience, been found the best in the arrangement of natural bodies. It is even applicable and commodious in other departments of science. Dr Cullen thought that no other should be followed in Mineralogy ; and he accurately observed it himself in his Nosology, in the arrangement of Diseases. He certainly did much in ascertaining the Classes, Orders, and Genera of Diseases, but always regretted that neither his opportunities nor his life were sufficient to ascertain the species. The same complaint may be applied to Mineralogy.”

## NOTE B, p. 4.

Dr DOBSON to Dr CULLEN.

“ DEAR SIR,

“ LIVERPOOL, 28th October 1764.

Mr Bostock, the bearer of this, is a young gentleman of Lancashire, who proposes to go through a course of medical studies at Edinburgh. He is well grounded in the languages, mathematics, and natural philosophy, and has made that quick progress in every thing he has yet applied to, which might be expected from uncommon talents and uncommon assiduity. As he is a stranger at Edinburgh, I take the liberty of thus mentioning him to Dr Cullen.\*

He will be much obliged by your directing him how he may most advantageously go through the several Colleges.†

I am much obliged to you for your last favour. Your medical information is always much to my advantage, and makes me very happy.

I have run over, in a very cursory manner, Sauvages's lesser work. The drawing out of a *Nosologia Methodica* is attended with many difficulties. To form the *Classes Morborum* from their proximate causes, i.e., the morbid alterations in the solids and fluids, would, I imagine, be the most simple, natural, and philosophical; but to execute this method in a satisfactory manner, there are many *desiderata* to which you are no stranger. To take the *Symptomata Morborum Pathognomica* for the distinguishing characters likewise promises fair; but to this a much more accurate history of diseases is necessary than we are yet supplied with. Besides, in the zoology, as well as in the botanical classes, the several subjects speak a language very intelligible; their appearances are obvious, and, consequently, the *characters* readily ascertained. But in the pathology, the symptoms depend upon the internal motions of

\* Vol. I., p. 644.

† The term College was at that time often used to signify a Course of Lectures.

the system, and many of them are sensations in themselves not easily to be expressed, and rendered still more vague by the imperfect and confused description of the patient.

The ten classes of Sauvages have their uses ; but in this method, I think, many diseases, which are in themselves analogous, fall under different classes, and are thus unnaturally and unusefully separated.

With best wishes for your health, and continued usefulness, believe me, dear Doctor, your affectionate and obliged friend and servant,

MATT. DOBSON.

P.S.—Pray what do you say to Hunter's *Transudation* ?”

#### NOTE C, p. 71.

#### CRITICISMS ON DR CULLEN'S SYNOPSIS OF NOSOLOGY.

This Note relates to certain criticisms pronounced by the late Dr Milligan on various terms, and some alleged grammatical errors in the *Synopsis Nosologiae Methodicae*. The criticisms relate mostly to modes of orthography ; and it is believed unnecessary now to notice them.

#### NOTE D, p. 88.

Reasons against Disjoining the Practice of Physic from that of Surgery, Midwifery, &c., by Legislative enactments, as assigned by Dr CULLEN and other Members of the Royal College of Physicians of Edinburgh, in 1769.

“ The study of the whole of Physic is certainly too much for one man ; there are few who attain to it, and we believe the most grave of physicians do not go half the length they ought in this matter. It might therefore be a project to divide the study of the profession still further, and confine practitioners to certain diseases only ; but it would be a foolish one, and would

produce practitioners not knowing in any disease. The principles are in common with the whole, and the application to different particulars serves to establish and to illustrate each other.

It is however true, that from particular dispositions and accidents, physicians will often attach themselves to some parts of their art in preference to others; and if they can persuade the public of their particular talents and acquirements, they will meet with encouragement in their particular branch of skill, and perhaps in that chiefly, by which means they become more knowing in it, and less exposed to avocations in other parts of the profession. This is, indeed, establishing certain persons for particular parts of the art, and perhaps in this way it is always with advantage to the public. But that such accidental determinations should be made a separate profession, which any man practising should be obliged to practise alone, and that no other physician should be allowed to practise the same, appears very improper. It is probable that practitioners will always be best formed by setting out upon a general plan of study, and encouraged by a view of a comprehensive employment, though we may allow particular determinations to take place afterwards.

If, then, a physician upon this footing shall be determined to the practice of surgery, or any part of it, no harm can ensue to the public; on the contrary, such a physician is likely to become more excellent in surgery than any man who, from the beginning, was determined to be a surgeon only. There is no advantage, therefore, to be got by forcing the separation in study, nor is there any necessity for any regulations to separate them in employment. The memorial mentions that one part of a profession may attach a man to one patient and place, and prevent him from attending others; yet there is no necessity for obviating such accidents, nor is it possible to do it by any regulations. We may as well enact, that no physician shall go out of town, because he cannot attend his patients in town and country at the same time, or limit every physician to a certain number of patients. We must leave to the public to obviate this inconvenience, which they certainly will, whenever they find

the patients of any one physician so numerous as to interfere with each other.

Another reason given in the memorial for this act is, that it is for the honour of the profession. But surely this College, who are all well versed in the history of physic, can lay no stress upon this argument, especially when it is well known that both now and formerly, there are and have been, in every country in Europe, physicians who practise surgery without the least derogation from their dignity as physicians ;—Ruysch, Heister, Deventer, Astruc, Chamberlane, Douglas, Nisbet, Sands, Bamber, Sir Richard Manningham, Sir David Hamilton, and others practising midwifery, were always on a level with any of their profession, and in rank above most of them, several of whom were Fellows of the Royal College of Physicians in London. We could say the same of some present practitioners both at London, Dublin, and elsewhere.

\*                      \*                      \*                      \*

\*                      \*                      \*                      \*

If the separation of Midwifery from Physic was the principal intention for passing this new act, it is certainly one of the most improper.

Midwifery is a part of surgery the most diversified that we know of, and the most requiring the general principles of physic. As a judgment in physic is often inseparable from the practice of midwifery, when it is not possible to have either the physician always at hand, or to render him useful unless he is exercised in the practice of it ; therefore it is the interest of mankind to have the two conjoined, if possible, in one person.

We are persuaded that the public will think it for their interest, in cases which are attended sometimes with so great and sudden danger, that physicians of the first rank should undertake the profession of midwifery, and that the Legislature will not suffer the College of Edinburgh to put a mark of contempt upon such Physicians by excluding them from their society."



## NOTE E, p. 104.

I avail myself of this opportunity of noticing that, by some Continental physicians, the fact of the notes added by M. Bosquillon being derived from Dr Cullen's lectures has been overlooked. Thus Marcus, in his *Entwurf einer Speciellen Therapie*, dritter theil, p. 205, says, "The French translator of Cullen has expressed himself in a remark upon blood-letting, and the fever of Erysipelas, so correctly, that it is deserving of being quoted here verbatim :—

'Blood-letting produces, especially in Rose of the face, a salutary revulsion. We cannot believe that it interrupts the course of the fever, that this is necessary to the bringing forward of the eruption, and that this must cease with the fever; because the fever continues so long as the eruption lasts, especially when the latter is violent. The practitioner must, as in other inflammations, also in Rose, draw blood, and repeat this blood-letting according to the degree of violence of the fever. The opinion that the fever is necessary to the bringing out of all the EXANTHEMATA has caused very great evil, because practitioners are thereby deterred from blood-letting, and have been led to the fatal employment of heating stimulating remedies.'

And again, when speaking of the abstraction of blood in Measles, after referring to Sydenham, Cullen, Monro, and Watson, Marcus says,—"The French translator of Cullen, who generally has annexed many excellent remarks derived from ample experience, adds,—

'Blood-letting is, after the establishment of the eruption, more beneficial than before that occurrence; although it may be likewise serviceable, when unfavourable symptoms, for instance, oppression in breathing, and violent fever, are present.'

## NOTE F, p. 113 and p. 117.

Vacca had previously objected to alleging that in Italy and Greece fevers often begin with heat. (Saggio, p. 130). See Notes.

And the same author denies that debility is always present in fevers. (Saggio, p. 131).

Testa inclines to think that miasmatic fevers commence with a sensible excess of vigour, not with a manifest languor of all the powers of life, though this may very speedily supervene. (Cap. xiii. 1).

#### NOTE G, p. 125.

This Note, which relates to certain mistakes of Vacca, Mason Good, and some other critics of the doctrines of Cullen, is considered to be unnecessary.

#### NOTE H, p. 146.

“ Subsequently to the period at which this notice of the dispute as to the contagious or non-contagious nature of Yellow Fever was written, my father, Dr Thomson, devoted a great deal of time to the investigation of the authorities and evidence on the two sides; and it is fair to say that his opinions, which had been strongly in support of the contagious view, underwent a great modification.”

W. T.

#### NOTE I, p. 156.

Dr JACKSON to Dr CULLEN, on the Connection between the Moon's Periods and the Invasion and Relapse of Fevers.

“ SIR,

EDINBURGH, *December 10, 1785.*

It was only the other day, though it had been published some time, that I met with Dr Balfour's Treatise on the Influence of the Moon in the Fevers of India. As I had observed, I thought, something of the same kind in a different part of the globe, the subject could not fail to interest me. In comparing, however, what Dr Balfour related with what I

thought I had seen, I find a considerable disagreement ; so considerable, indeed, that if the facts on which I depend had not been taken in a manner so little liable to deception, I should have been much disposed to believe myself wrong. As it is by your desire and recommendation that this pamphlet by Dr Balfour is reprinted, the subject, I presume, seems curious to you. I shall, therefore, in a few words, and a few words may do, take the liberty to lay before you the little I have observed with regard to it.

It may not be improper, however, before going farther, to mention that the observations I am going to relate were made at Savannah-la-Mar, in the Island of Jamaica, and that they were principally, though not wholly, collected from a company of soldiers of the 60th Regiment, who were quartered at that place.

Savannah-la-Mar, as you probably know, is situated in a plain on the shore of the sea. The plain extends into the country a dozen of miles or more in most directions ; and the fort or barrack of the soldiers is on a small point that runs out into the sea a few hundred paces. The principal disease of Jamaica, I may remark, is a fever of the remittent kind, which has been observed to be more regular and more remarkable in its remissions at this place than in most other parts of the island.

Before I went to the West Indies, which was in the beginning of 1774, I had read Dr Lind's Dissertation on the Putrid and Remitting Fever of Bengal. The observation he makes of the disposition that fevers have to relapse at the new and full moon, and at eclipses, appeared to me then a curious fact ; and some time after my arrival in Jamaica, I remember to have mentioned it to a gentleman who had practised many years in that island,—a man of observation, too ;—but he did not acknowledge ever to have observed anything of the kind ; I then concluded it was probably confined to India. It happened, however, in a few months, that a person, with whom I was nearly connected, fell ill of a fever. The fever was soon removed, but it returned again after a few weeks, and this return happened to be near the time of full moon. It was then

that Dr Lind's observation occurred to me, and I likewise recollect a circumstance that I had been utterly at a loss to account for, viz., that when the soldiers of the garrison were seized with fevers, two or three of them were usually seized on the same day, and this perhaps did not happen above once in a fortnight. It was thus that the hint was first suggested; and from this time I began to consider new and full moon as some way connected with the invasion, as well as the relapse of fevers; but it was more than a twelvemonth before I had obtained a distinct idea on the subject.

In order to ascertain what the degree of connection was, and how far it extended, in the beginning of 1776, I provided myself with an almanack, and in the blank leaf of the almanack marked down the name of all those who were attacked with fevers, and the day and hour at which they first perceived the approach of the disease. At the end of the year I found thirty cases of what I would call proper remitting fever, twenty-eight of which had happened on one or other of the seven days that precede new or full moon. The year following I continued the same method, and, at the end of the year, found that of twenty-eight, twenty-two had happened in the above-mentioned period. Besides these there were likewise marked in the almanack a good many day-fevers and slight feverish disorders, the greatest number of which had likewise happened in the second and fourth quarters of the moon, though it must be confessed the proportion was not so great as in those I consider as proper remitting fever.

It may further be remarked, that though the seven days preceding new and full moon, or the whole of the second and fourth quarters, are to be included in what may be called the sickly period, yet the number of fevers that attacked on the days previous to the fourth was proportionally small. In general, they were fevers of a violent kind, and happened to people who had been exposed to violent occasional causes. The fourth day preceding new or full moon was a day remarkable for the invasion of fevers, more remarkable than any other in the period except perhaps the day of new or full moon itself.

It should likewise be observed, that the invasion of fevers

is not equally nearly connected with new and full moon in every season of the year. In the dry months, which is generally the healthy season in Jamaica, the attack of a fever is seldom observed to be more than four days before the new or full moon. In the wet season it is greatly more irregular, as well as by the season of the year. The regularity of this appearance seems to be much affected by situation of place. Among the soldiers who were quartered in the barracks, and very little exposed to any violent occasion of disease, the invasion of fevers was most closely connected with the new and full moon; so closely connected with them, indeed, that though it had been my custom to go to the barracks every evening, to prevent my being sent for, perhaps, when I was going to bed, yet in the year 1777, I seldom went unless in the second and fourth quarters of the moon, and I scarcely recollect to have been once sent for.

In spring 1778, I arrived at New York, and joined a regiment that was quartered on York Island. I continued the observations I had begun in Jamaica, and before the end of the year, of about a hundred cases of intermitting fever, I found eighty to occur in the second and last quarters of the moon.

It ought to be remarked, however, that in the beginning of the summer, the influence of the moon was most remarkable. In September, and part of October, when the disease was very epidemic, it was greatly diminished.

In the end of the year, the regiment embarked for Georgia, where, being almost constantly in the field, I found it difficult to continue the observations. I observed, however, in general, that in what might be called the healthy season, the invasion of fevers was much connected with new and full moon; but in those months when the fever became epidemic, which it sometimes did to an extraordinary degree, this connection was sometimes so much lost, that I could hardly trace it.

The most material circumstance in which, what Dr Balfour relates, differs from what I thought I had observed, is in the period in which he observes fevers to come on. He confines it to the three days immediately preceding or following new or full moon. I would consider it as the second and fourth quar-

ters, when the moon approaches fast to full or change. Of the eight cases that happened in the first and third quarters, three, indeed, happened but a few hours after the moon had changed; the other five were several days.

I should likewise be much disposed to differ from Dr Balfour as to the opinion he forms of the crisis of fever. What may happen in India it would be presumption in me to say. This, however, I am confident of, that sixty cases of fever, whose beginnings I had marked to an hour, and which I may say were left to their natural course, terminated every one of them on an odd day. This is a bold assertion, and would not be true without some explanation, without making an allowance for anticipation of types, or simplifying such as are complicated. Of the critical days, I have found the seventh the most remarkable. Of sixty cases, nearly thirty terminated on that day, about ten or twelve on the fifth, and the same number on the ninth.

I am, Sir, with the greatest respect, your much obliged humble servant,  
 ROBERT JACKSON."

NOTE I. (*lis*), p. 185.

Dr FRANCIS HUTCHISON to Dr CULLEN.

" MY DEAR SIR,

DUBLIN, 4th August 1779.

Every occasion of reviving a correspondence with one I so truly and gratefully esteem, gives me sincere pleasure. This letter is really a tribute of gratitude for the information and pleasure I have received from the perusal of the second volume of your First Lines, which came to my hands a few days ago. From the first volume I received the highest satisfaction last year; from this still more. Your doctrine of Hæmorrhagy is throughout, without compliment, the most ingenious, satisfactory, and solid piece of pathological reasoning I have ever met with; and, further (which is not always the effect of ingenious theories), I am sure no man can have carefully perused it and reflected upon it, without becoming a better

practitioner in such diseases than he was before. I flatter myself I comprehend tolerably your views; but how much more fully should I understand them, had I the happiness of hearing your own illustrations, and filling up of the outlines!

From my situation here, and the different forms of disorders in different countries, I find myself forced, though reluctantly, to differ from you somewhat in regard to the Miliary Fever. I have twice within these twenty years seen fevers attended with this eruption, from the reigning epidemic constitution in this city, and that not for one season only, but for several years in succession. When it first reigned, perhaps the regimen of some of our leading practitioners was rather *hot*; but of late years that has not been the case. I have seen this eruption appear under the coolest regimen,—without profuse sweating; and if I can in any degree trust to my own judgment, as truly critical as the eruption of the Small-pox. From the preceding symptoms of the fever I could almost certainly predict it. These were, principally, a particular kind of anxiety and affection of the respiration, and a pale greenish yellow colour of the urine, which, in the cases that terminated favourably, continued to be discharged in very large quantity to the end of the disease, without depositing. Slight degrees of moisture on the skin were attended with the pricking sensation previous to the eruption. I have seen the most violent and almost deplorable symptoms suddenly vanish upon a plentiful eruption about the 8th or 9th day, and never again return. Dr Quin and I attended a most striking case of this kind a few months ago, about which, with the doubts on this subject fully in our view at the time, we could not entertain the smallest suspicion. All I mean to infer from this is, that the prevailing constitutions in Scotland are very different from those with us; and that, perhaps, observations on acute diseases, subject to variations from epidemic or endemic constitutions, are to be considered as *Ephemerides* local and temporary. I am told you have the low Nervous Fever very frequently in Edinburgh. We seldom have seen it here for many years past. Putrid and Petechial Fevers are uncommon with you. Here they have been most common since the Miliary Fever became more rare.

I am become almost an infidel in regard to contagion immediately from the body of the sick, if not kept in confined air. A happy disposition of mind, you will say, for a physician. About three years ago, some prisoners brought into court from a crowded gaol, communicated the usual fever to a great number of persons. All this time there was not any fever in the gaol, and the prisoners who were tried were all in health. We reckoned thirty-five cases of persons that day in court to whom we had been called, persons of some note, of whom seventeen died. But there was not a single instance of any person in these thirty-five houses having caught the infection from the sick, although some of them, being people of distinction, had numerous attendants. Another circumstance struck me on this occasion. There were numbers of people who daily resorted to the gaol, and to the rooms where the prisoners were confined, and staid in them for a considerable time, with impunity. I made particular inquiries as to this fact, and can be certain of it. *Quære*, Is it necessary that this miasma should be mixed with fresh air in order to its exerting its effects when applied to the human body? All the histories of contagion of this sort that I have heard were connected with this circumstance; a foul person brought into *pure air* among healthy people. Why else should not a person going into the foul confined air of the gaol run a greater risk of infection than by standing near one coming from this foul air into an open airy hall? Does this poison act most certainly and powerfully in a diluted state?

I have at present under my care a patient in an uncommon disorder, a Diabetes, and, as far as my experience goes, in an uncommon kind of it. The disease is in the *quality* of the urine, not the *quantity*, which seldom has exceeded that of her drink, and is often under it. But it is sweet, like a solution of new honey, without any appearance of the natural ammoniacal salt, and, when kept, appears first acescent, then grows mouldy, and acquires a kind of musty putridity, just as described by Dr Dobson of Liverpool. An uncommon aridity of the skin, from a total want of perspiration, led me to attempt the restora-



tion of this excretion by Antimonials and Opiates. In this I have succeeded, with very manifest advantage, my patient having recovered strength, appetite, and rest. *Adhuc sub judice lis est.* You shall at a future time have a particular account, when I can determine the permanence of these effects.

I beg my most affectionate compliments to good Mrs Cullen. I hope she has not forgotten me. I can never forget her. I am sure you have had much satisfaction in my two young friends Quin and Cleghorn. I am ever, most sincerely and affectionately, yours,

F. HUTCHISON.

*P.S.*—Pray spare me half an hour.”

#### NOTE K, p. 198.

See Cullen's First Lines, § 1353.

See also an article “On some Convulsive Diseases common in certain parts of Scotland,” inserted in the third vol. of the “Edinburgh Medical and Surgical Journal,” p. 435, under the head of ‘The Inquirer,’ No. XI.

That this affection is not yet wholly extinct, appears from a paper by Dr Dewar of Dunfermline, entitled *History of a Singular Convulsive Disease affecting Five Children in one Family*, and inserted in the fifty-second vol. of the same Journal, art. v., p. 87, July 1839.

Dr FARQUHARSON of Dundee to Dr CULLEN, giving an account of the *Louping Ague* of Forfar.

“DEAR SIR,

DUNDEE, Sunday.

Some weeks ago I was advised by Dr Ogilvy at Forfar in Angus, of a very singular species of disease that had lately appeared in his neighbourhood. His description of it raised my curiosity so much, that I went to see the cases a few days since; and I could not be easy without communicating to you some account of this uncommon disorder.

The first family I saw had two children seized with it at the same time, and they had been at this time three weeks ill. One, a girl of thirteen; the other about five. Some minutes after I was in the house, one of them was attacked with this disorder, and immediately fell upon her knees; with her head bent back betwixt her shoulders, her neck projecting outwards and very turgid, her eyes not at all disordered nor fixed in this posture, she remained half a minute; after which she got up in great confusion, ran to a large table, leaped up to it at once, though three feet high; her tongue making a circle in her mouth and producing a confused, blubbering noise;—her upper lip the only part of her face any way distorted. When on the table, she tried to get off her shoes, after which she jumped three or four feet perpendicular for some minutes. By this time the other was seized in like manner, and went through the same operation. Both ran from the table to the head of the bed; from this to the couples and joist of the house. In short, they performed most of the postures of Sadler's Wells; nor did either of them seem in the least danger of falling. I was told that the servant maid, aged nineteen years, had gone away with the same disorder, but lived in the neighbouring town. I was soon satisfied of the truth of this, by her arrival in one of the fits of this disorder. She had run half a mile quicker than a young man, her uncle, could follow her, though she was thrown down several times in the position described above, for some seconds. The house-top seemed to be her hobby-horse, where, though the wind was immensely loud, she jumped three, yea, four feet, perpendicular for a considerable time. The others observed her, and I saw their agitations more violent; they all went together, and seemed to dispute the places they were to leap upon, tumbled over one another; and one of them lying upon the floor, upon her back, raised herself up to her feet upon the table. All this time they have their senses; answer, as well as the contraction of the mouth permits, your questions distinctly; but say, the disease, by them called *It*, forces them to do so and so, and they must obey it. The eldest girl is very regular in her habits; and the moment the fit is over, she goes

to work, complaining of hunger and thirst. I observed their pulses, which were quick, small, and irregular. These fits sometimes continue half an hour, sometimes longer; but when two of them meet, they leap hours together, mimicking one another, and going over the same process exactly. They are all seized with a kind of heaviness and dulness for some days; then they begin to complain of pains in the præcordia; and particularly most of them say it begins in the little finger of both hands, going gradually from thence to the head.

The people here believe it contagious, four or five families being in the same situation. Their bellies keep easy, and all of them sleep in the night time, and look healthy and well. The Jumpers' country being nigh to Mr Skene of Skene's House, I sent him to visit them, and he saw this terrible scene in perfection. Several boys have laboured under it, and there are two married women in the same way. The tingling of bells, or even of the tongs, brings it on; or the sight of any of their distressed neighbours.

Your opinion and advice about this *Monstrum Novum*, and to what class you think it properly belongs, will be highly satisfactory. I have ordered Setons, Vomits, and the strongest of the fetid nervous medicines, but yet cannot say anything of their effects. You have been long promising a visit to this neighbourhood. Could you not spare as much time as come over and see this strange disease? The country calls it the Louping Ague. Lord Gray, presently with you, will inform you of the distance, when you tell him they live upon the side of the Hill of Caterthun. Dear Sir, yours sincerely,

WILL. FARQUHARSON.

Pray let me hear from you soon."

NOTE L, p. 212.

The Collection of Serous Fluid giving rise to Dropsy is made,

I. By Effusion, depending upon

1. Increased Exhalation, occasioned by

A. Resistance to the Return of the Blood in the Veins.

a. By obstructions.

aa. In the lungs.

bb. In the heart.

cc. In the liver.

b. By the posture of the body.

c. By a general plethora in the veins, from

aa. Obstructed hemorrhoidal flux.

bb. Obstructed menstrual flux.

cc. Obstructed serous evacuations.

d. By obstruction of particular veins, by

aa. Polypous concretions in the veins.

bb. Compression,

α. By tumours in the coats of the veins.

β. By tumours external to the veins.

αα. Skirrhus.

ββ. Steatomatous.

γγ. Bulk of the uterus in pregnancy.

δδ. Bulk of the water in Ascites.

B. Resistance to the passage of fluids by the excretories.

C. The increased impetus of the blood in the arteries,

a. By external violence, as in strains and bruises.

b. By fever.

D. By relaxation of exhalants,

a. In palsy.

b. In general atony, as in chlorosis and cachexy.

E. By an increase in the proportion of the watery parts of the blood,

a. By the quantity taken in.

b. By the watery parts of the blood, retained in consequence of interrupted excretions.

c. By the grosser parts being drawn off ;—

aa. Red globules and gluten by hæmorrhagies.

bb. Gluten by serous and purulent evacuations.

2. The rupturè of lymphatics.

3. The rupture of vesicles and sacs.

II. By the retention and accumulation of fluids naturally poured out in consequence of *diminished absorption*, from

1. The first part of the document discusses the importance of maintaining accurate records of all transactions and activities. It emphasizes the need for transparency and accountability in financial reporting.

2. The second part of the document outlines the various methods and techniques used to collect and analyze data. It includes a detailed description of the experimental procedures and the statistical analysis performed.

3. The third part of the document presents the results of the study. It includes a series of tables and graphs that illustrate the findings of the research. The data shows a clear trend of increasing activity over time.

4. The fourth part of the document discusses the implications of the findings. It suggests that the results have significant implications for the field of study and may lead to further research in this area.

5. The fifth part of the document concludes the study. It summarizes the key findings and provides a final statement on the importance of the research.

Allen; his name being recorded, in Dr Cullen's handwriting, in a list of that class, which has been preserved. The name of John Brown is recorded in the list of the Chemical Class for the first time in session 1761-62; and for the second and third time, in the two subsequent sessions.

There is no doubt, therefore, that Dr Cullen had given to Mr Brown free admission to his lectures at this time,—namely, in 1760, 1761, 1762.

At this time commences the romance of the story.

It is stated, more or less distinctly, both by Dr Beddoes and by Dr C. Brown, that Dr Cullen found Mr John Brown's knowledge of the Latin language useful, and made him a sort of amanuensis or Latin secretary. This is entirely a piece of invention. The only capacity in which Dr Cullen employed Mr John Brown was as tutor or private teacher to his children, to assist them in the preparation of their lessons, and their Latin exercises. Dr Thomson was assured by Dr Cullen's son, the late Archibald Cullen, Esq., the London Chancery lawyer, that this was the sole duty performed in the family by Mr Brown. Four members of the family further informed Dr Thomson, that Mr Brown never acted as amanuensis, but that Dr Cullen either wrote most of his manuscripts by his own hand, or availed himself of the aid of one or other of his sons or daughters. Henry Cullen often performed this duty.

The statement that John Brown assisted Dr Cullen by his knowledge of the Latin language, is contradicted by the fact that Dr Cullen had delivered, as parts of academical duty, discourses or addresses in the Latin language, long before he became acquainted with Mr Brown; and though the first two editions of the Synopsis of Nosology were published subsequently to this event, no proof is given that the Latin style of this work had in any way ever been submitted to Mr Brown's revision.

Mr Brown indeed became, during the period of his acting as private tutor, very intimate in the family of Dr Cullen. But this intimacy appears not to have been of the kind that would justify the inference, that Dr Cullen ever treated him in any other manner than as a kind and benevolent patron would treat

a young man of learning,—endeavouring to improve his condition in life by the exertion of such talents as he possessed, and who was capable of turning his knowledge and attainments to useful purposes. Mr Brown had early given proof of considerable aptitude for exciting amusement; and, in attaining this purpose, he was not scrupulous and delicate as to the means employed. Dr Thomson was informed on the best authority,—that, namely, of at least four members of the family,—that whenever Dr Cullen was out at dinner, or otherwise from home, the children of Cullen requested Mr Brown to dine with them; and on these occasions he was in the habit of amusing them with all sorts of low buffoonery. It is altogether a thing incredible, that a person possessing such qualifications as those now mentioned was likely to be referred to by Cullen for supplying any deficiencies, or rectifying supposed errors in Latin composition and expression.

On the 10th of February 1773, Dr John Gregory died suddenly and unexpectedly,—an event which placed Dr Cullen in sole possession of the Chair of Practice of Physic, and caused a vacancy in the Chair of Institutions of Medicine. The candidates for the vacant office were,—Drs Gregory Grant, William Buchan, Andrew Duncan, Daniel Rutherford, and Alexander Monro Drummond; and of these, the Patrons who had previously consulted the Medical Professors, upon their selection being made known, appointed Dr Drummond, in accordance with this selection, as Professor of the Institutions of Medicine.

Upon this occasion no mention is made of John Brown having been a candidate; and it is certain, from the records of the Town Council, that he was not a candidate. Yet Dr Beddoes has stated, that Mr Brown said, “Cullen had promised him his interest for the first vacant chair in the College; but when the late Dr Gregory died, instead of using his interest in Brown’s favour, he did the reverse,” and has, without any good foundation, represented this occurrence as one of the earliest indications of misunderstanding between Dr Cullen and Mr John Brown, and of alleged hostility and unfriendliness of the former to the latter person.

Dr Monro Drummond, who was at Naples, in Italy, at the

time at which he was appointed Professor, did not appear in time to undertake the duties ; and for three successive sessions these duties were performed by substitute ; first, in 1773 and 1774, by Dr Francis Home, the Professor of Materia Medica, at the request of the Professors, and with the sanction of the Patrons ; and secondly, in 1774–1775 and 1775–1776, by Dr Andrew Duncan, by appointment of the Patrons. The Chair of Institutions then became again vacant, in 1776, by the resignation of Dr Drummond ; and this event is represented by Dr Beddoes and Dr Cullen Brown to have brought out, in a decided form, the deceitful and insidious conduct of Dr Cullen to Mr Brown, and the consequent open hostility of the latter. Mr Brown is by them represented to have been a candidate for the vacant office, to have been promised by Dr Cullen his aid and interest ; and if he did not obtain the chair itself, to be employed as interim lecturer.

The whole credibility of this statement, however, is destroyed by one part of it, in which it is said that Dr James Gregory, the successful candidate, was travelling on the Continent. This is not the fact. The records of the Town Council prove that this gentleman appeared on the day of his nomination in the Council Room, and accepted of the office,—a fact which proves that his appointment was certainly and confidently anticipated. Dr Gregory accordingly entered on the performance of the duties of Professor of the Institutes of Medicine in November 1776 ; and it was upon this occasion, and by the express request of Dr Gregory, that Dr Cullen published the second edition of his *Outlines of Physiology*, for the use of the pupils who were to hear the prelections of Dr Gregory. There was no occasion, therefore, for any substitute to teach the Class ; so that the whole of this statement by Dr Beddoes, and which is repeated in substantially the same terms by Dr Cullen Brown, can be looked upon in no other light than as a statement totally at variance with known and well-established facts.

Dr Beddoes asserts that Mr Brown was a candidate for the vacant chair in 1776. This he may have been ; but of the fact no proof is adduced. Dr Thomson, who had examined the



Records of the Town Council, states that they contain no references to any applications from candidates, a circumstance from which it may be inferred, as already stated, that it was understood that Dr James Gregory was certainly to be nominated. The only other person thought of for the office was Dr Andrew Duncan; but it is not said that even he was a candidate. It may be regarded as a certain fact, that whether Dr Cullen had on this occasion promised his assistance or not in favour of Mr John Brown, no assistance that he could promise would have been of the slightest avail against a candidate such as Dr James Gregory, the son of the former professor of the Practice of Physic. It is, again, incredible to any one who knows the characters of the persons, and the usual mode in which these offices are disposed of, that Dr Cullen could have given such a promise.

As the greater part of the subsequent representations regarding the misunderstanding between Dr Cullen and Mr John Brown rest upon the story now adverted to, it is evident that it is unnecessary to go further into the examination of the matters referred to. Lengthened, confused, and rather declamatory, statements are given by both the biographers, and especially by a pupil of Mr Brown's,—namely, Mr Jones,—regarding other matters, but especially an application of Mr Brown to be admitted into the Philosophical, subsequently the Royal, Society. According to the statements of Mr Jones and Dr W. Cullen Brown, Mr John Brown, though he entertained suspicions that Dr Cullen was now unfriendly to him, yet had not come to an open rupture; but he employed this proceeding, of applying for admission to the Philosophical Society, as a test of the sincerity or insincerity of his Patron; in other words, if he were not admitted, he was to ascribe this to the influence and enmity of Dr Cullen. It is stated by Mr Jones, that Dr Cullen, after the petition for admission had been presented, strongly advised him to withdraw it, stating that it would be opposed, and “assigning a frivolous pretext for the opposition which threatened it.” This Mr Brown declined doing. The petition for admission was, it seems, opposed by a majority. Mr Jones adds,—“The consequence of his rejection was an immediate rupture between him and Cullen.”

The interview at which Dr Cullen endeavoured to persuade Mr John Brown to withdraw his application for admission into the Philosophical Society was the last occasion on which Cullen attempted to advise him. Cullen saw that he had lost whatever influence over Mr Brown he might have possessed, and ceased then to hold any communication with him; it is said even to mention his name. Upon the subsequent course of this rupture it is unnecessary to dwell.

Mr Brown, it was said, was aided and abetted in his subsequent proceedings against the reputation and doctrines of Dr Cullen by Dr Gilbert Stewart and Mr William Smellie, printer in Edinburgh. The former has been made sufficiently well-known, by the account in D'Israeli's *CALAMITIES of Authors*, as the assailant of Dr Robertson, the Principal of the University of Edinburgh, the abusive critic of the Grammar of Dr Adam, and the author of the unjust and uncandid review of Dr Henry's *History of Great Britain*.\* He was a person of considerable learning and much acuteness, great powers of writing, but of debauched habits; and, in point of fact, he shortened his life by his excesses. Mr Smellie was a man of great natural talents, and much industry in the acquisition of knowledge, and rendered himself known by his work on the *Philosophy of Natural History*.

Neither of these men could be safe advisers. Gilbert Stewart was a person in whom all sense was blinded, all judgment perverted, by passion, disappointed ambition, ill temper, and frequent, if not incessant use of intoxicating drink. Smellie was a person of more reputable character. But he allowed himself to be swayed by some foolish prejudices and mistaken notions, and often listened more to the suggestions of feeling than the dictates of calm and dispassionate reflection.

The greatest enemy to John Brown was evidently John Brown himself. As early as 1763, which was two years before he

\* *Calamities of Authors*, under head *Literary Hatred*, or *Miscellanies of Literature*. By I. D'Israeli. A New Edition. London, 1840. 8vo.

The reader may also consult *Memoirs of William Smellie*, late Printer in Edinburgh, &c., by Robert Kerr, Edinburgh, 1811; in which are given several facts and letters not published by I. D'Israeli.

married, and only four years after his arrival in Edinburgh, he had contracted habits of indulgence and intemperance, which had then made, according to the testimony of a friend of a correspondent of Dr Beddoes, serious inroads upon a constitution originally firm and vigorous. He became also a sort of boon companion among the students; and his society was courted for his convivial powers and his faculty of amusing. This poor ambition appears to have proved his bane. After he established his boarding-house, which, it is said by Dr Beddoes, on the authority of Mr Wait, was quickly filled with respectable boarders, his convivial habits were greatly increased. He lived in a style too expensive for his means; and in the course of two or three years he became bankrupt. Yet was this the person for whom, we are gravely informed, it was to be expected that Dr Cullen was to exert his influence in obtaining the office of Professor of the Institutes of Medicine, and whom it was expected the Philosophical Society would be willing to receive as an associate.

As to his discovering a new System of Medicine, all that can be said is, that the use of the term "discovery" is totally misapplied. It is not by such speculations as those which John Brown threw out, that new systems are constructed. It seems to have been expected by various persons, that Dr Brown, for he had graduated on the 21st September in 1779, would have reformed his habits of intemperate indulgence, as he advanced in life. This expectation was not realized. Dr Brown continued to spend days, and, it is said, nights, in the company of those medical students and others who liked his society and conversation, and who all agreed that he was a most amusing and diverting companion; and so long as he remained in Edinburgh, which was till 1786, no change in his mode of living and acting, no improvement in his habits, took place. This course of life could not be continued without serious injury to health, to character, and to finances; and though he still had pupils and auditors to his lectures, he was in straitened circumstances, and in debt. Meanwhile, he had been forming, in 1780, the resolution of going to London, with the intention of there promulgating the Principles of the New System of Me-

dicine; and in reference to this plan, we find Dr William Saunders of London,\* when writing to Dr Cullen upon other matters, makes the following mention of Dr Brown in a postscript.

“ I had lately an opportunity of seeing a curious correspondence from Dr Brown to his friend in London, in which he expressed a determined resolution to come to London and prosecute a plan of reading Clinical and other Lectures, and in which he had the audacity to mention your name in a manner not becoming your rank and character in your profession.” Dr Cullen’s answer, dated 30th October 1780, is in the following terms :—“ Your postscript hardly told me any thing but what I expected. The man you spoke of has a very wrong head, and a still worse heart.”

“ *Fœnum habet in cornu ;*

\* \* \* \* \*

*Hunc tu Romane caveto.”†*

To London Dr Brown accordingly went some years afterwards,—namely, in 1786. But his career there was short and miserable. His habits had followed him; and it is recorded, that while he was lecturing, he was wont to take alternately sippings of spirits and laudanum. He expired suddenly, but not unexpectedly, on the seventh day of October 1788, in the fifty-third year of his age,—a mournful example of talents misapplied and learning abused; of neglect of the common rules of prudence; of time and opportunities that might have been employed in augmenting the amount of useful knowledge, wasted in propagating specious fallacies and pernicious errors.

The following chronological arrangement of the principal events in the life of John Brown will show the relative position of each.

Age of John Brown.	Events and Occurrences.	Year.
	John Brown was born in . . . . .	1735
24,	Came to Edinburgh in . . . . .	1759
26,	Attended Dr Cullen’s Lectures on Materia Medica,	1761
27,	Attended Dr Cullen’s Lectures on Chemistry,	1761–62
28–29,	Attending Dr Cullen’s Lectures on Chemistry ; twice, . . . . .	1762–64

\* Author of works on Diseases of the Liver, Mineral Waters, &c.

† *Horatii Sermonum, Lib. I. IV. 35–65.*

30,	Married ; and commenced a house for reception of boarders, . . . . .	1765
	First edition of Cullen's <i>Synopsis Nosologiae</i> pub- lished, . . . . .	1769
35,	J. B. was still attending Medical Lectures in Uni- versity, . . . . .	1770
35 or 36,	First Attack of Gout, . . . . .	1770 or 1771
	Publication of First Edition of Cullen's Institu- tions of Medicine, or Elements of Physiology, . . . . .	1772
	Cullen's Lectures on Materia Medica in 1761, sur- reptitiously published at London, . . . . .	1772-1773
41,	J. B. President of Medical Society, . . . . .	1776
41,	Date of Dr Gregory's appointment as Professor of Institutions ; 19th of June, . . . . .	1776
	First Lines of Practice of Physic by Cullen, Vol. I., published, . . . . .	1777
	Second Edition of Cullen's Elements of Physio- logy, published by request of Dr Gregory, . . . . .	1777
41,	Second attack of Gout in . . . . .	1777
43,	Begins to deliver Lectures on Practice of Physic in Edinburgh, . . . . .	1778
44,	Graduated at St Andrews, on 21st September, . . . . .	1779
45,	President of Medical Society ; <i>second time</i> , . . . . .	1780
45-46,	ELEMENTA MEDICINÆ of J. Brown first published, . . . . .	1780
	Lecturing and Teaching in Edinburgh, but pro- posing to go to London, . . . . .	1780
51,	Went to London, . . . . .	1786
52,	Outlines published ; (Observations on Old System of Physic), . . . . .	1787
53,	Translation of Elements published, . . . . .	1788
53,	Death in London, 7th October, . . . . .	1788

## NOTE N, p. 223.

In this Note it was proposed to give the Hand-Bill or Ad-  
vertisement of the Lectures of Mr John Brown. But this is  
now considered to be unnecessary.

## NOTE O, p. 317.

Under this reference Dr Thomson had collected from Werlhof,  
Bohn, and other writers, various extracts illustrating the belief  
in the existence and powers of the *Vis Medicatrix et Conserva-*

*trix Naturæ* among physicians, long before the time of Cullen. But this it is considered to be superfluous to insert.

NOTE P, p. 440.

To this reference, no note, or materials for note, were found. But it may from the context be inferred, that what was intended to be stated related to the reasonings of Tommasini in his Work on the Yellow Fever of Leghorn in 1804, on the proofs of the existence of inflammation in that epidemic and similar febrile diseases.\* Something has been already stated in the text immediately following this reference, on the line of argument adopted by Tommasini; and it is now conceived to be unnecessary to recur to the subject.

NOTE Q, p. 443.

To this reference nothing was found to correspond except some detached extracts from the *Examen* of Broussais; and these extracts relate rather to certain criticisms by Broussais on Brown's doctrine of Debility, than to the question as to the existence or non-existence of local affection in Fever. It is further to be remarked, that as the peculiar doctrines proposed and maintained by Broussais upon the subject of Fever, and its connection with, and dependence upon, local disorder and inflammation, are well known and easily attainable from works in every one's hands, it is unnecessary, after what has been said in the previous Note, to add more on that subject. Those who desire to study more fully and attentively the peculiar doctrines of Broussais, will find them expounded in his various writings, especially the following.

*Recherches sur la Fievre Hectique.* Paris, an. XI. i.e., 1803.

*Histoire des Phlegmasies Chroniques, fondée sur de nou-*

\* *Richerche Patologiche sulla Febbre di Livorno del 1804.* Sulla Febbre Gialla Americana, &c. Del Professore Giacomo Tommasini. Prima Edizione Napoletana. Napoli, 1817. § 53, pp. 68-70.

velles Observations de Clinique et d'Anatomie Pathologique. Cinquieme Edition. Paris, 1838. 3 vols. 8vo.

Examen des Doctrines Medicales et des Systemes de Nosologie, precedé de propositions renfermant la Substance de la Medecine Physiologique. Troisième Edition. Paris, 1829-1834. 4. forts volumes in 8vo.

English readers who are unwilling, or find it inconvenient, to study the French works now mentioned, may find a sufficiently clear and detailed view of the principles and doctrines of Broussais in the following work.

Conversations on the Theory and Practice of Physiological Medicine ; or, Dialogues between a Savant and a Young Physician, a Disciple of Professor Broussais, &c. Translated from the French. London, 1825. 8vo.

#### NOTE R, p. 495.

Under this Note it was proposed to give some account of the subsequent course of this departure from Brunonian doctrines by the Italian Physicians, and of the principal persons who have figured in this part of the History of Medical Science and Practice in Italy. But the subject is one which is daily losing interest, and possesses, indeed, little utility, unless in showing the ignorance and rashness of Physicians and their adherents, and their love not so much of novelties, as of old doctrines in new garbs, and under new denominations. The new Italian doctrine has indeed been scarcely able to preserve the semblance of existence, even in the Peninsula, before the advancing influence of the doctrines of Mesmerism, Homœopathy, and Hydropathy; and, practically speaking, may be said to be extinct.

Those who are desirous to understand the steps, by which the Italian practitioners came to secede from the principles of John Brown, may find, if not all the requisite information, at least as much as is sufficient, in the work of Tommasini already quoted in the previous Note (P) and in the following treatises.

1. Dell' Infiammazione et della Febbre Continua Considera-

zione Patologico-Pratiche. Di G. Tommasini, Professore di Clinica Medica Nella P. Università di Bologna, &c. Pisa, 1820. 8vo.

2. Della Nuova Dottrina Medica Italiana. Prolusione alle Lezioni di Clinica Medica Nella P. Università di Bologna Per l'Anno Scolastico, 1816-1817. Del Professore Giacomo Tommasini. Bologna, 1817.

The Notes appended to this performance are particularly instructive, and show what has been already mentioned, the vacillation of the mind of Tommasini as well as his learning.

3. Sullo Stato Attuale della Nuova Patologia Italiana; Considerazioni del Professore Giacomo Tommasini, Esposte in una Nota Al Discorso di esso Sull' Influenza dell' Opinione in Medicina. Con alcune recentissime correzioni et aggiunte da esso fatte. Raccolta, Tomo IX., p. 49.

4. The Treatise of Guani, Opuscolo del Controstimolo e delle Malattie Irritative, has been already referred to in the text. It is proper here to add, that the Notes on this Treatise are highly instructive, and will be read with advantage and interest.

Raccolta di Opere Mediche Moderne Italiane. Tomo V. Bologna, 1827. P. 77.

5. Saggio Della Teoria Medica del Controstimolo. Dal G. D. V. Mantovani. Ibidem, Tomo V. P. 1.

6. Su la Dottrina della Vita; Saggio di Maurizio Bufalini, M.D. Ibidem. P. 122.

7. Various Letters of Speranza; in Raccolta Tomo Settimo pp. 99, 124, 133; and his Clinical Report for 1823-1824, under the following title.

Anno Clinico-Medico Compilato da Carlo Speranza, &c. Parma, 1825.

8. Fondamenti di Patologia Analitica di Maurizio Bufalini; esenate; Medico in Patria, &c. Edizione Terza. Tomo I. Pesaro, 1828. Tomo II. Pesaro, 1830.

9. Prolegomeni Clinici per servire d'Introduzione Teoretica; allo Studio Pratico della Medicina. Del Cavaliere V. Luigi Brera, Professore P. O. di Terapia Speciale e di Clinica Me-



dica nell I. R. Università di Padova, &c. &c. In Padova, 1823. 8vo.

Tommasini states in Note 48 of *Ricerche Patologiche*, that the History of the Epidemic Fever of Genoa of 1799 and 1801 by Rasori was at first received by several physicians with indifference; by others it was read with repugnance, in consequence of the considerable opposition which it presented to received opinions upon the nature and the diatheses of certain Fevers. At the time, however, at which he wrote, we presume 1805, it began to be read with greater interest; and he adds that it had already produced great advantages by inculcating useful circumspection in the treatment of certain Fevers, which previously were on too slight grounds considered to be produced by Debility, and which were accordingly too boldly treated by means of stimulants.

Tommasini appears to think, that he never can say enough of the inflammatory character and tendency of Fevers and several other diseases, and of the necessity of employing blood-letting, antimonials, refrigerants, cathartics, and similar lowering agents.

#### NOTE S, p. 502.

#### ON THE OPINIONS AND DOCTRINES OF TOMMASINI.

Notwithstanding the multiplicity of the writings of Tommasini, it is extremely difficult to understand, in all instances, the exact nature of the doctrines to which he is most favourable; and it is altogether impossible, in the short space within which such an abstract should be confined in the present work, to give an account at once just and intelligible of his physiological and pathological doctrines.

It is not in any degree doubtful, that Tommasini began his professional life by being a warm and rather injudicious admirer of the principles and doctrines of John Brown, and had much confidence in the practice recommended by that person. When, however, he had been some time in actual practice, and

had become familiar with the phenomena, the course, and the effects of disease, he speedily perceived that these principles were greatly at variance with, and often completely contradicted by nature. This caused a contest and struggle in the mind of Tommasini, the effect of which was to give rise to an oscillation of opinions and reasonings, and a degree of inconsistency and unsteadiness in therapeutics, which appears in the whole of the writings which he has published during a space of not less than thirty years.

Tommasini had commenced his professional life with very considerable confidence in the doctrines of Brown, namely, that all diseases were referable to two classes only, Sthenic and Asthenic; but as, in imitation of Brown, he considered the majority of diseases which presented external symptoms of Debility to be Asthenic, and to require the use of Tonics and Stimulants; he accordingly treated them by withholding the use of evacuants, and by administering opium in different forms, camphor, musk, alcohol, wine, and other diffusible stimulants, and not unfrequently Cinchona in various shapes, and other Tonics and Astringents. This would not have been wrong, if Tommasini had remembered, that the external signs of weakness do not always indicate the necessity for the use of stimulants and tonics; and if he had adverted to the proper period of the disease, and to the fact, that in the commencement of acute diseases, though symptoms of great Debility in the animal functions are present, yet stimulants and tonics are not only useless and hurtful, but the less that is done by medicine, except the employment of moderate and necessary evacuations, and the re-establishment of suspended secretions and excretions, it is the better for the patient, and he more easily gets over his disease. When Tommasini recognised the hurtful effects of this method of treatment, he naturally and consistently found it requisite first to modify and then to change considerably the theoretical principles which had been the foundation of the therapeutic method. *Asthenia* or Debility was then not only removed, in his mind, from the prominent and commanding eminence on which it had been placed by Brown, but the principle which had been held as a Divinity, was with little ceremony

thrown down and trampled under foot. It was, in short, characteristic of the mind of Tommasini, like many converts, to rush to the opposite extreme; and whereas, previously most diseases were believed to be *Asthenic* or dependent on Debility, now the majority of diseases requiring and admitting of medical treatment were deemed *Sthenic*, or dependent on increased force in the vital actions.

Various circumstances contributed to effect this change in the Pathological principles and the Therapeutic rules of Tommasini. But he seems to have been forcibly impressed by the bad effects of the stimulating treatment of Fevers so early as August 1791, by an attack of Fever which he then suffered in his own person. As the account given by Tommasini shows in some degree the train of reasoning by which he arrived at his most important conclusions, it seems proper to give the passage in his own words.

“ And to turn to this sthenic character of Bilious Fevers, from which I have deduced one of the arguments to enforce the belief equally of the original diathesis of Yellow Fever (§ 78), it is not only by another method of cure, and by the observations cited, that I have found it my duty to recognise it as such. My own observations, and the methods of treatment practised by me as well before as after the reception of the New Medical Doctrine in Italy, have convinced me of this. Previous to this doctrine, the treatment of Fevers of this order was generally trusted to the use of purgatives, emetics, and subacid drinks; and a great number of this sort of Fevers it has fallen to my lot either to treat or to see treated by my Preceptors by this same method. An exception there was when the remission of the fever, rather decided, produced entirely by evacuants, suggested sometimes to try the use of Cinchona, or the impatience of the sick demanded this remedy; or for some particular symptom oftentimes, Tonics were interposed between the doses of purgatives; as in order to allay the nervous disturbances there was no difficulty in making use of anodynes and volatile medicines even in the height of the disease, and in the midst of the lowering treatment; and as, in order to procure sleep during the night, there was not the smallest scruple in prescribing opiates. Now I can declare, that I have

observed at that time, and have recorded the fact in my practical memoirs, that the use of Cinchona had caused not a few patients to retrograde from convalescence already commenced to a new course of the disease, accompanied perfectly with the first symptoms (§ 48); that wine and cordials were not tolerated in this order of Fevers; that Camphor, which was sometimes employed in order to promote perspiration, remarkably aggravated the pain of the head and the Fever; and that opiates, far from procuring sleep as desired, rather increased delirium and sleeplessness. How strongly was I convinced of these practical truths by the circumstances of a Bilious Fever, by which I was attacked in Mantua in the summer of 1791 (§ 48), and for which I was treated in Luzzara by my most dear friend Dr Luigi Navaroli."

" Filled still with the theoretical principles of the schools, and practising only for two years in this Hospital (the Hospital of Mantua), I was perpetually agitated by those illusions, which experience gradually dispels, and I was animated by that confidence which the practice of any master, of any time, and of any doctrine, tends, if I mistake not, to diminish rather than to increase. It appeared to me then, that Art possessed still more power than I believe it to possess at present. I urged more than once my physician to try again strong doses of Peruvian Bark, after sufficiently copious evacuations had been procured; and I further compelled him at the same time to try the effect of Sedatives in a liberal dose, in order to free me from the most cruel symptom by which I was tormented, namely, sleeplessness. But the febrile exacerbations, instead of being mitigated under the use of bark, were redoubled in strength, and recurred more violently; and Laudanum and Cynoglossum drove from me sleep instead of favouring it; while the delirium, heat, headache, and the throbbings of the arteries increased under the use of anodynes, tonics, or the generous wine which my friends provided for me. My medical friend, alarmed by a second relapse which these attempts and irregular proceedings had produced, consulted the writings of the ablest practitioners. He read over the work of Dr Grant, and with these observations in his hand, induced me to adhere strictly to the use of mild

purgatives and antiphlogistic remedies, by means of which the disease was finally subdued."\*

Tommasini goes on to say that Gastricism, that, is mere Gastric and Bilious disorder, lost its ancient influence, and he was further convinced that all fevers were not, as Brown pretended, produced by Asthenic Diathesis ; (§ 92). He then takes an extended and elaborate view of the effects of the methods of treatment pursued by English, American, and French physicians in Yellow Fever and Bilious Fevers.

Tommasini himself virtually admits that the phenomena of the Leghorn Epidemic of 1804, and the description of that disease given by Palloni, were powerfully instrumental in directing his thoughts and reasonings in this line. That disease was, he inferred, highly and distinctly inflammatory, presented decided marks of *Phlogosis* ; and he then set himself to prove that all the American, the English, and the French authors, who had written on it from personal observation, either said the same thing, or gave testimony which could only be interpreted as denoting the existence of inflammation. When you look for any given thing, you are pretty sure to find it in some way or other. Tommasini looked for *Phlogosis* or Inflammation in Yellow Fever and Bilious Fever ; and he found it not only in these maladies but in many more.

Inflammation was then the grand essence of all acute diseases, and of a large number of chronic diseases. It seems to be a law of human thought, that, in order to reason and to speculate about any thing, or on any subject, in a satisfactory manner, it is necessary to fancy some existence, some being or essence which the mind lays hold of, or tries to lay hold of ; and this being done, all reasoning, formation of inferences, and establishment of conclusions becomes easy. *Phlogosis* was a stimulus ; it was an irritant agent ; and so long as it was present, it caused excitement and irritation in the organs and textures, probably in the fluids of the human body. To counteract, to oppose, or at least to abate and alleviate, the effects of this Stimulus, it became necessary to have recourse to the use of a

\* *Ricerche Patologiche sulla Febbre di Livorno del Anno 1804, &c. &c.* Del Prof. G. Tommasini. Napoli, 1817. § 91, pp. 136-139.

certain class of agents ;—medicines, drinks, and external means. Forthwith these were called Contrastimulants or Controstimulants. These contrastimulant remedies consisted in blood-letting, general and local ; the use of Refrigerants, as Nitre, Bitartrate of Potass, Acetate of Potass, Citrate of Potass, Acidulous Drinks ; Evacuants and Laxatives, especially the Neutral Salts ; Antimonials, Cool Drinks, and all depressing means.

In this manner was established that system of precepts and practices to which Tommasini gives the name of the **NEW ITALIAN MEDICAL DOCTRINE**.

A singular part of the whole of this history is, that Tommasini appears sincerely to believe, that the doctrine which he advocates as the New Italian Medical Doctrine is really new and original. This belief can be received as indicating only one of two things, either a very considerable degree of confidence in the ignorance of readers, or a large amount of ignorance on the part of the learned professor. It would not be difficult to prove, that this doctrine is as old as Hippocrates, and has been maintained not only by his own countryman Baglivi, and many others, but has been a standing doctrine among English physicians for fully a century and a-half. It was a doctrine maintained not only by Sydenham, Andrew Brown, and Dover, but at later periods by Huxham, 1750–51 ; Cleghorn, 1751 ; Sir John Pringle, 1752 ; Francis Home, 1759 ; Brocklesby, 1764 ; Donald Monro, 1764 ; George Fordyce, 1770–1794 ; Grant, 1771 ; James Sims, 1773 ; and almost every good physician since their time ; and if it fell into partial abeyance, it was revived as to Fever, and received new illustrations and confirmations by Sutton, Irvine, Jackson, Burnett, Bateman, Clutterbuck, Percival, Mills, and not a few others, and cannot in any sense of the term be pronounced to be new.

These men did not, indeed, speak or write much about their theoretical opinions. Some of them are so silent on this head, that it is impossible to discover whether they had any doctrinal principles or not. With all of them, theory and doctrine hung so loosely, that if they influenced, they never seem to have embarrassed their practice. But all of them observed the phenomena and tendencies of diseases. All of them considered

what was likely to do good to the patient, and employed these means skilfully and promptly, and all of them eschewed whatever they perceived did harm or interfered with the powers of nature in producing recovery.

Huxham, Cleghorn, Home, Sir John Pringle, Donald Monro, Brocklesby, Grant, Sims, and Fordyce, all prescribed venesection in Fever and Inflammatory Diseases. All of them administered nitre, many gave cream of tartar, and several of them antimony. All gave aperient medicine and the neutral salts. All avoided tonics, stimulants, and nutritious food during the presence of the symptoms indicating active Fever or Inflammation, active or subacute, or even chronic. In proof of the correctness of this statement, it cannot be necessary to give extracts from the works of these physicians, which are perfectly accessible to every one. But it may not be useless to refer to two of those works which are perhaps less read.

It has been mentioned in the text, that Dover, who had been a disciple of Sydenham, when he afterwards accompanied Woodes Rogers and Dampier to South America, describes shortly, after the storming of Guayaquil in the year 1709, an acute Fever among the seamen, to which he gives the name of Plague. It is needless to say, that this was not Plague, properly so called, but merely the Ardent Bilious Fever of these regions, which often attacked European sailors; Remittent Fever, in short, or Bilious Fever, or Yellow Fever, or Sailors' Fever; (*Fieure Matelotte*). This Fever Dover treated with great success, and almost no mortality, by means of copious blood-letting, and the use of cooling acid drinks. He afterwards relates how in England he, in like manner, treated very bad malignant Spotted Fever, apparently Typhous Fever, by copious blood-letting and refrigerant remedies, and with like success.

The work of Dr James Sims on the Febrile Disorders of the County of Tyrone in Ireland during the years 1765, 1766, 1767, 1768, and onward to the early part of 1772, is full of instances of the antiphlogistic and lowering method of treatment having been employed by him, with the same beneficial results.

In the Febrile Disorders of that County, Dr Sims made use

not only of blood-letting, but Antimonials, especially Tartrate of Antimony and Kermes Mineral, the Neutral Salts, Cream of Tartar, Acids in general, and every part of the cooling and lowering method of treatment. In several epidemics he exposed the patients freely to cool and cold air for some time. And he disapproves in decided terms of the use of Cordials and Stimulants in the early stage of Febrile Disorders.\*

Let it not be forgotten, though it seems like repetition and iteration to say, that Cullen had with great care reduced the practical results given by these physicians into a system of general principles, and by combining with them those derived from his own experience, laid before medical practitioners, in his *First Lines*, a most useful Compendium of Special Therapeutics, the careful study of which would have prevented Tommasini from falling into the mistakes and misrepresentations which his writings present.

In what sense the peculiar Italian Medical Doctrine, which Tommasini professes to promulgate, can be styled New, or in what sense it is entitled to be called a Discovery, those who are acquainted with the facts now stated, as illustrating the History of Medicine both in England and on the Continent during the latter half, it may be said three-fourths, of the eighteenth century, will be capable of forming a just understanding.

NOTE T, page 508.

The Rev. Dr JOHN WALKER to Dr CULLEN in regard to the expected vacancy in the Professorship of Natural History.

“ DEAR SIR,

MOFFAT, 7th February 1778.

I received this week, for the first time, the accounts of poor Dr Ramsay's illness, and of his being thought past hopes of recovery. When you mentioned to me, some time ago, your view of the place for Harry,† I looked forward with regret at

\* *Observations on Epidemic Disorders, with Remarks on Nervous and Malignant Fevers.* By James Sims, M.D. London, 1773. 8vo. *Passim*; but especially pages 154, 156, 158, 159, 204, 208; and on the use of Cold Air, 211-215.

† Henry Cullen, M.D.



the very possibility of any interference between us, yet I thought it more than likely that before any opening in that place could happen, both he and I might be otherwise and better appointed. At the worst, in the event of a competition, I was conscious that, on my part, it would proceed without any infringement of that sincere and entire regard which, for many years, both from duty and inclination, I have uniformly maintained for your person, character, and family. I was incapable of taking umbrage at such a friend for a preference, not merely to his own son, but to his merit. And I relied on your candour and generosity for my appearing as a competitor, by fair and honourable methods, for an office which had been for years the great object of my life.

With these sentiments, which I trust you will not condemn, I closed with the proposals of some of my friends, who have offered to interest themselves in my favour. And being this day informed that Harry has declined appearing as a candidate, I shall with more freedom and alacrity proceed to solicit others. How far the other candidates may have gone I know not, and I may perhaps be too late. But as I am assured that no promise of the place is yet given, I should reflect severely on myself afterwards, did I not do what is in my power at present. I have been in several quarters disappointed. I am for several reasons sick of this place, and anxious to leave it, and was even thinking to give it up, if I could have but procured a chaplainry to go with a regiment to America.

With my kindest service to Mrs Cullen, Bob, Harry, and all the family, I ever am, with the greatest regard, dear sir, your most obliged and affectionate humble servant,

JOHN WALKER."

Dr Robert Ramsay of Blackcraig, physician in Edinburgh, and Professor of Natural History in the University of Edinburgh, died on the fifteenth day of December one thousand seven hundred and seventy-eight; (1778).\* There is, regarding the date of this occurrence, some discordance in the accounts

\* Scots Magazine, Volume XL., for 1778, p. 686.

given by different persons. Kerr, in his *Memoirs of William Smellie*, the Printer, who became a candidate for the office, states that Dr Ramsay died in 1775.\* But several of the letters relating to this event which he quotes are without date; and though two are dated (No. 114, 116), yet we are led to infer that the date was conjectural, and inserted afterwards. No mention of the death of Dr Ramsay is made in the *Obituaries of the Scots Magazine* for 1775; and, on the other hand, it is distinctly recorded, in the volume for 1778, as having taken place on the 15th day of December one thousand seven hundred and seventy-eight; (1778). Mr Bower, the historian of the University of Edinburgh, places it in 1778, and the appointment of Dr Walker in 1779;† and it is certain, from the records of the *Senatus Academicus* of the University, that Dr Walker was inducted on the third day of November 1779.

Dr Walker was said to be a person of eccentric character and singular habits. When at Moffat, it was said by one of his parishioners, that he spent the week in hunting butterflies, and made the cure of the souls of his parishioners a bye job on Sunday. Whether this be true or not, he appears to have had little of the elevation of mind which characterises the genuine naturalist. He evinced the most extreme jealousy of the attempts of Smellie to teach Natural History in any form, and great hostility against all who favoured his pretensions.

After the death of Dr Walker, his Executors published, in 1808, *Essays on Natural History and Rural Economy*, in one Volume 8vo. In these Essays the author treats of Botany, Arboriculture, the Natural History of the Highlanders, Mineralogy, Geology, Zoology, and Statistics; and though the information is often superficial, yet the work is not uninteresting, and may be read with interest.

\* *Memoirs of Life, Writings, &c. of William Smellie*, by Robert Kerr, in two volumes. Vol. II., p. 90, &c. Edinburgh, 1811.

† *History of the University of Edinburgh*, Vol. III., p. 226.

## NOTE U, p. 510.

Letter of Dr CULLEN to the Honourable HENRY DUNDAS, Esq.,  
afterwards Lord Melville.

" SIR,

*November 12th, 1786.*

I was at Hillhead this morning to have the honour of waiting upon you, and of talking to you about a little business; but not being so happy as to find you at home, I beg leave to trouble you with this letter to explain the business I intended to have spoken of.

My worthy colleague, Dr Hope, died yesternight, whereby the office of Professor of Botany is become vacant, and I intend to propose my son, Dr Henry Cullen, as his successor.

I have always been ambitious of your favour; I have thought myself highly honoured and obliged by the instances of it I have frequently received, and beg the continuance of it on this occasion, but think it incumbent upon me to lay the grounds of my application before you.

My son has, for a great part of his life, made Botany his particular study, and has given proofs of his proficiency in it. My colleagues of the Faculty of Physic allow me to say they are ready to testify their opinion of his being well qualified for the office he now seeks; and a gentleman, who, it will be acknowledged, is one of the first botanists in Britain, and who has had access to know my son's progress in that branch, is ready to give the most ample recommendation of his fitness to fill the Botanic Chair.\*

Presuming upon all this, I venture to add, that I flatter myself upon some pretension to the favour of the administrators of the city. For now thirty years I have served as a Professor in their University, and have devoted my time and labour to the promoting of the reputation of this school of medicine. With what success my labours have been attended, and how much they have contributed to the increase of the students at

\* This is supposed by Dr Thomson to refer to Dr John Walker.

the University, it is perhaps unbecoming for me to say; but I trust I may be allowed to claim some merit with the town of Edinburgh, and some right to their favour.

It is natural for me to wish that a son, whom I carefully trained to the study of Physic, should be a Professor in the same University with myself; and when his qualifications to fill the found vacant chair are acknowledged by those who are the best judges of them, I hope it will not appear presumptuous in me to solicit for him.

To be honoured with your favour and patronage upon this occasion, would, I know, be of the utmost importance to my son's success; and I hope you will forgive me for requesting it in the most earnest manner. Your doing so will confer a very high obligation upon me, and I have the honor to be, &c.

(Signed) WILLIAM CULLEN."

The Honourable HENRY DUNDAS, Esquire, to Dr CULLEN, in reply to the foregoing letter.

" SIR,

12th November 1786.

I have just now the honor of your letter. Nobody feels stronger than I do the importance of proper appointments to the medical chairs in the University of Edinburgh; and I have uniformly acted upon that principle; and, in the present instance, so far as I may have any concern in the business, shall continue to do so.

At the same time, under that reserve, I should act uncandidly not to inform you that I have, ever since the appointment of Doctor Walker to the Professorship of Natural History, retained a strong disposition to assist Dr Rutherford in his views to obtain a Professor's Chair. He was at that time strongly recommended to me by many persons whom I wished particularly to oblige; but the current of opinion was so strong in favour of Dr Walker, I was under the necessity of supporting him. Dr Rutherford's behaviour was so gentlemanlike on the occasion, it impressed me with the feeling I have expressed, and

which I wish to act upon in the present instance, if no public grounds operate against it.

I have the honor to be, Sir, with great respect, your most obedient and humble servant,

HENRY DUNDAS."

NOTE W, p. 514.

It has been supposed by some, that a misunderstanding or unfriendly feeling had taken place between Cheselden and Dr James Douglas. Of such an occurrence I find no distinct proof either in the medical history of that period, or in that of the relations subsisting between Mr Cheselden and Dr James Douglas. The manner in which this idea has arisen, it appears to me, is the following.

There were at that time, 1725–1736, two persons of the name of Douglas, brothers; the one Dr James Douglas, the Lecturer on Anatomy, a physician; the other, his brother, Mr John Douglas, Surgeon to Westminster Hospital, a man of eminence as a surgeon, and particularly as a lithotomist, in which character he was a great patron and practitioner of the High Operation.

Dr James Douglas, so far as I can perceive, continued during life on the most friendly terms with Cheselden, and is well known as one who took very great pains to explain both the anatomical advantages of the lateral operation for Lithotomy, as practised by Cheselden, and to show its success when compared with the High Operation. There is no proof in the whole course of their common history that either of these men looked on the other with any other feelings than those of good and friendly understanding. Dr Douglas was a physician, and took the deepest interest in the improvement of the Lateral Operation, and in making known its peculiar advantages, chiefly from his desire to benefit the science and practice of surgery, and from a desire, which appears in all that he did, to render everything that passed through his hands as perfect as possible, and to explain to others wherein this superiority consisted.

There was no desire to claim for himself any peculiar merit in this proceeding, and certainly none to deprive Cheselden of the reputation to which he was most justly entitled as a cautious and skilful operator. It was more as an anatomist and surgical anatomist than in any other character, that Dr James Douglas appeared as the advocate of the Lateral Operation. Nor can we perceive in the circumstance that Dr James Douglas and Mr. Cheselden both lectured at the same time on anatomy, any evidence of jealousy or envy in either of these two men.

Dr Douglas states, in terms sufficiently clear and plain, the reasons which induced him to give the account which he did of the operation of Mr Cheselden. When that operation became the subject of conversation abroad, especially in France, and several accounts of it were made public, though several of these accounts, of which he says he had seen three or four, contain many of the essential parts of Mr Cheselden's operation ; yet, in every one of them, something is wanting. Dr Douglas resolved, therefore, for the credit of English surgery and of the operation itself, to give, once for all, Mr Cheselden's whole method of proceeding. And he expresses the opinion, that this detail will be of some use even to the Parisian surgeons themselves, notwithstanding that they have the best opportunities for making the necessary experiments for every operation ; but infinitely more so to surgeons in other places, both at home and abroad, who have not had such advantages.

He farther adds, that he was obliged to Mr Cheselden for the chief materials of his paper ; and ' as he has been so kind as to communicate to me, with the greatest readiness and without reserve, all the particulars which I could not have come to the knowledge of, I am confident that none will pretend to dispute, but what I here describe is his operation, and his whole operation.'

Another object of Dr Douglas was, to present a clear and distinct account of the anatomy of the parts concerned in the operation, a matter which, it must be allowed, was given only in a general way by Cheselden.

That all this was done in the most friendly manner towards the great surgeon of St Thomas's Hospital, London, will be admitted by every one who remembers what testimony Che-

selden himself bore to the correctness of Dr Douglas's account, and that he subsequently, after the death of Dr Douglas, made the following spontaneous statement.

'I had,' says Mr Cheselden, 'this account of *Herniæ* from the late Dr Douglas, a most industrious anatomist, very communicative, and much to be relied on. \* \* \* The present cases I have from Mr Hunter (Dr William Hunter), a pupil of his, who dissected many of those, which were shown me by the Doctor, and who, to all the good qualities of his great master, has added that of true philosophy.' \*

In making these remarks, we forget not, that our friend the late Dr Yelloly, with the view of rectifying what he conceived to be an erroneous statement by Dr Douglas, thought it incumbent on him to republish, in the Fifty-First volume of the Edinburgh Medical and Surgical Journal,† the account which Cheselden himself published of his method of operating, in the Fourth Edition of his Anatomy. To republish that account was perfectly proper, as it contains, regarding the patients operated upon, some exact details not given in the ordinary editions. But while we make this admission, it must be added, that Cheselden himself, though he lived several years after the date of the publication of this Fourth Edition, and published one, or rather two editions, never afterwards republished this account; and the descriptions which he has left in the Fifth and Sixth Editions, which must be taken as the most authentic, contain no complaint against Dr Douglas, and no remark, but merely an account of the incisions and manner of cutting, so as to get with safety into the bladder. There is, in short, no indication, either at this time or subsequently, that any misunderstanding or dislike had separated these two eminent men; and we find that when Dr Thomson, in 1808, republished the account of Cheselden, with the illustrative portion of Dr Douglas's postscript, he shows that he had no belief

\* The Operations in Surgery of Mons. Le Dran, Senior Surgeon of the Hospital of La Charité, &c., translated by Thomas Gataker, Surgeon. With Remarks, Plates of the Operations, and a Set of Instruments. By William Cheselden, Esq., &c. London, 1749. 8vo, p. 463.

† April 1839. P. 346.

that any misunderstanding had taken place, but, on the contrary, expresses himself pleased in seeing Cheselden bearing testimony to the great services of Dr Douglas, and to the excellence of his character in general.

It was different with Mr John Douglas, the Surgeon, the brother of Dr James Douglas. He had formed some peculiar dislike against Cheselden, and took various opportunities of evincing this feeling, but more especially by writing a severe and uncandid review or attack upon the large work of William Cheselden upon the Bones. This work, which certainly proved the diligence of Cheselden in the study of Osteology, and his knowledge of the morbid states of the Bones, John Douglas calls a late pompous work, and examines it in a spirit which shows that he must have been at a great loss to find in it anything seriously wrong or defective.\*

What might be the causes which led to this hostility on the part of Mr John Douglas to Cheselden it is at this distance of time not easy to decide. Conjecture and probable supposition, from certain facts given in the writings of Mr John Douglas, are all that are left. It is not unlikely that Mr John Douglas, who was the advocate and performer of the High Operation,† which had been first practised and then given up by Cheselden for the Lateral Operation, was displeased at seeing, that the former was neglected, while the latter was practised by the most eminent surgeons. With considerable talents, and a sufficient amount of anatomical information, Mr John Douglas appears to have been a person remarkably different in disposition and character from his brother Dr James Douglas, and to have indulged in those feelings of envy and jealousy, which are occasionally observed to tarnish the character of men belonging to liberal professions. Mr John Douglas, in short, appears to have been one of those moral enigmas which not unfrequently appear in the history of Surgery, of a person of superior attainments, and belonging to a profession esteemed liberal,

\* Remarks on a late pompous work, &c. by John Douglas. London, 1735.

† *Lithotomia Douglassiana*: or, a New Method of Cutting for the Stone; first practised by John Douglas, Surgeon, F.R.S., and LITHOTOMIST to the INFIRMARY at Westminster, &c. London, 1723.



having recourse to the degrading arts and vituperative language chiefly, if not solely, confined to inferior traders.

I cannot conclude this Note without adverting to the great merits of Dr James Douglas as an anatomical teacher. Besides the "*Descriptio Comparata Musculorum Corporis Humani et Quadrupedis*," the *Bibliographiæ Anatomicæ Specimen*, to which Portal has assigned the highest character, he gave in 1730 an excellent description of the Peritonæum, one of the earliest examples of structural and general anatomy, and which evidently contains the germs and rudiments of the essay by his pupil, William Hunter, on the Cellular and Adipose Membrane, of which it must be regarded as the precursor.\* He was occupied for several years in preparing a series of elaborate engravings of the Bones; and he had been engaged up to the time of his death in preparing Drawings of the Uterus, gravid and unimpregnated, and the contiguous parts. His death took place before he was able to get these engravings published; and before the Drawings of the Uterus were engraved; and both came into the possession of Dr William Hunter, who succeeded him in teaching Anatomy in the Anatomical Theatre at Covent Garden, London. They are now in the Hunterian Museum at Glasgow. The copperplates of the engravings of the Bones I had an opportunity of examining there in 1857, by the kindness of Dr Allen Thomson; and I think no one can look through them, without perceiving that Dr Douglas must have been not only a good anatomist, but possessed considerable skill in regulating the pictorial representation of the objects. The engravings of the Bones appear to be fully as good as any of those published in the last century. The appearance of Cheselden's elegant work on the same subject may have interfered with the publication of Douglas's plates; but it cannot be doubted, that the latter would have formed a most useful contribution to the illustration of Osteological Anatomy.

\* A Description of the PERITONÆUM, and of that part of the MEMBRANA CELLULARIS which lies on its outside. With an Account of the True Situation of the ABDOMINAL VISCERA in respect of these two Membranes. By Dr James Douglas, Physician in Extraordinary to Her Majesty, Honorary Fellow of the R. College of Physicians. London, 1730. 4to.

The drawings of the Uterus and neighbouring organs are numerous, elaborate, and instructive. Dates are marked upon some of them, extending from the year 1709 to 1738. It may be supposed that the investigations upon which these drawings were founded, of which Dr Hunter must have acquired a knowledge in his association with Dr Douglas previous to his death, may have first directed Dr Hunter's thoughts to the importance of publishing a work on the Gravid Uterus. The drawings for the plates of Dr Hunter's great work on this subject published in 1774, the originals of which, chiefly by Rymdyke, are also preserved in his museum at Glasgow, appear, from the dates which are marked upon a number of them, to have been executed chiefly at three distinct periods, viz., from 1750 to 1754, from 1764 to 1766 and about 1770. They are of a much larger size, and in a much higher style of art than any of those belonging to Douglas which have been preserved. They are also chiefly representations of cases and dissections which came under Dr Hunter's or Mr John Hunter's more immediate observation. It is, nevertheless, proper to say that the drawings of Douglas manifest an intimate acquaintance with the subjects which they illustrate, and present a considerable body of most useful information.

Dr James Douglas died on the 1st of April 1742, in the sixty-seventh year of his age, leaving a widow and two children.

#### NOTE X, p. 517.

Letter from Sir BENJAMIN C. BRODIE to Dr CRAIGIE regarding the Windmill Street School.

Though I had from various sources information regarding the School of Anatomy in Great Windmill Street, yet I thought it incumbent on me, when speaking of Dr William Hunter, to apply to Sir Benjamin Collins Brodie, at that time (1855) the only survivor of those who had been connected with that establishment. The following answer, which I quickly received

from Sir Benjamin Brodie, will, I think, be perused with interest, both on account of the subject and of the writer.

“ MY DEAR SIR,

14 SAVILLE ROW, *June 8, 1855.*

I am happy to answer your inquiries as to the Windmill Street School, as far as I am able to do so.

The building in Great Windmill Street, including the House, Museum, Theatre, and Dissecting Rooms, was erected by Dr William Hunter, who continued to lecture there until he died. For some time he had been assisted by Hewson, afterwards by Cruikshank. I have understood that Cruikshank had been originally engaged by William Hunter as librarian, and that he acted in that capacity for some time before he began to teach anatomy.

After William Hunter's death the lectures were delivered by Dr Baillie and Cruikshank conjointly, until the former retired (I believe) in the year 1798. They were then delivered by Cruikshank conjointly with Mr Wilson. Cruikshank died shortly afterwards,\* and was succeeded by his son-in-law Mr Thomas, who, however, was engaged chiefly in giving demonstrations in the dissecting room, and delivered very few of the lectures.

In 1805, Mr Thomas left the school; and Mr Wilson proposed to me, being then a senior pupil, to succeed him as Teacher and Demonstrator of Anatomy in the Dissecting Room.

In 1808, having become a good deal engaged in private practice, Mr Wilson proposed to me that I should deliver a part of the Lectures in the Theatre; and accordingly I delivered a considerable portion of each course during the next five sessions. At the end of that period I retired from the Anatomical School, but continued to deliver my Lectures on Surgery in the same Theatre for two or three years longer.

From this time the Anatomical Lectures were delivered by Mr Wilson and Sir Charles Bell conjointly; the latter, however,

\* Mr Cruikshank died on the 27th June 1800, at the age of 55. His death was the result of apoplexy from hemorrhage of the left hemisphere of the brain, causing much laceration of cerebral matter.

taking much the larger share of them. A few years afterwards, Mr Wilson having retired altogether, the school became the sole property of Sir Charles Bell, who, however, was assisted in teaching Surgery by his brother-in-law, Mr John Shaw.

On the establishment of University College (then known as the London University), Sir Charles Bell left off teaching anatomy in Windmill Street, being succeeded there by Mr Mayo and Mr Cæsar Hawkins.

King's College was founded in 1830. But two or three years elapsed before the building was completed, so as to admit of Anatomy being taught there. Mr Mayo then transferred his Lectures to this new Institution, of which he was the first Anatomical Professor ; and thus the School of Anatomy, founded by William Hunter, came to a conclusion.

Until the year 1807, the Lectures in Windmill Street were illustrated by the preparations in William Hunter's Museum. On the Museum being at that time removed to Glasgow, Mr Wilson purchased the building in Windmill Street from Dr Baillie, who had inherited it from his uncle. Mr Wilson being aware that he should lose the use of William Hunter's preparations, had previously provided another collection of preparations to supply their place. These were almost entirely made by his own hands, and were admirably adapted for the purpose. I may observe, by the way, that Mr Wilson was a most profound and accurate anatomist, and a most excellent teacher. He was, indeed, *facile princeps* among the London Anatomists of that day. By the arrangement made between him and Sir Charles Bell, his anatomical preparations became Sir Charles Bell's property, and with extensive, and some very valuable additions made by the latter, are, as you are aware, now in your metropolis.\*

I have the pleasure of sending you, by the same post, a Discourse delivered by myself at our College of Surgeons some years ago, which refers to the principal points respecting William Hunter as a teacher, with which I have become acquainted.

\* This collection is preserved in the Museum of the Royal College of Surgeons, Edinburgh.

If I can give you any further information on the subject in which you are interested, I shall be most happy to do so. I am, dear Sir, yours very truly,

B. C. BRODIE.

Dr CRAIGIE."

In July 1831 I accompanied the late Dr Herbert Mayo to visit the theatre and other apartments in Great Windmill Street. The Museum was then in a transition state as to its contents, which were undergoing the process of being conveyed to the building then erecting for King's College. The greater part of the preparations, especially those most delicate, had been removed, and only the most bulky, chiefly skeletons of animals, remained; the last relics of what had been left after the transference of the Bell Collection to Edinburgh. But even with all this dilapidation and desertion before and around me, it was impossible to avoid thinking with esteem and veneration on the talent, the zeal, and the energy of the man who, singly and unaided, had been able to establish a place of instruction, the influence of which was so powerful, so extensive, and so beneficial.

#### NOTE Y, p. 674.

On the condition of Ormiston Hill at the time (1778), at which Dr Cullen acquired that property, the following observations are made by the Rev. William Cameron, minister of the parish of Kirknewton, in the first Statistical Account; and as Mr Cameron was a personal observer of what he describes, his testimony may be regarded as well-founded.

"He (Dr Cullen) was a great master in the scientific branches of husbandry; a consummate botanist; and possessed a full-formed and correct taste in the fine arts. These attainments appeared conspicuous in his operations at Ormiston Hill. In the year 1758 the Doctor, after finishing his course of chemistry, delivered to a number of his particular friends and favourite pupils nine lectures on the subject of agriculture. In these few lectures he, for the first time, laid open the true principle

concerning the nature of soils, and the operation of manures. These were discoveries entirely his own, and which have since been made known to the world by a variety of channels, though without any notification of the source from whence they proceeded. The justness of these principles he demonstrated by his practice on the lands of Ormiston Hill, which, though naturally of an ungrateful soil, rendered worse by immemorial bad management, and situated in an unfavourable climate, he raised in a few years to a surprising degree of culture and fertility. Early in life he was a proficient in botany, and was the first person in Scotland who understood the Linnæan system, and recommended the study of it to his pupils at a time when it met with much opposition from others. In his gardens and pleasure-grounds at Ormiston Hill he formed an extensive collection of rare trees, shrubs, and herbaceous plants. The cultivation of these, and the accurate determination of their species, afforded him always an agreeable relaxation from the more serious studies and labours of his profession.”\*

Since the time of Dr Cullen,—since 1792, when Mr Robert Cullen disposed of the property of Ormiston Hill, nearly sixty-seven years have elapsed ; and during that time, and especially of late years, several important changes have taken place at Ormiston Hill. Though the present proprietor, Archibald Wilkie, Esquire, who is also possessor of Ormiston, continued to occupy, till 1852, the old mansion inhabited by Dr Cullen, and had built an extensive establishment of coach-house, stables, and other offices, he erected, about six hundred yards to the westward, on the elevated western bank of the Dingle, which overlooks the Caledonian Railway and the country beyond to the north, a large, commodious, and tasteful mansion in the modernized Tudor style of architecture, and which forms the finest object in the landscape in that neighbourhood. The old house has been since entirely given up to be occupied by servants.

\* The Statistical Account of Scotland. Drawn up from the communications of the Ministers of the different Parishes. By Sir John Sinclair, Bart. Volume Ninth. Edinburgh, 1793. P. 416.

Over the Dingle or ravine, also, a substantial stone bridge has been thrown, so as to connect Ormiston Hill on the east, with Ormiston on the west of the ravine. From this bridge the spectator obtains an excellent view of the Dingle upwards to the south, and downwards to the north, though in the latter direction the view is limited by the winding nature of the ground; and he may see that it is justly entitled to be called a Bosky Bourne, as it forms the boundary between the two properties.

This is the proper place to remark, as the circumstance tends to throw light on some features in the character and habits of Cullen, that on a gate at the northern and lower outlet of this Dingle, Cullen had affixed from the soliloquy of the usurer ALFIUS, in the second Epode of Horace, the words "*PRÆCUL NEGOTIIS*," evidently showing how closely he participated, not in the hypocrisy of Alfius, but in the praises of the country life, which the poet has in language so expressive put in the mouth of this person. It is, indeed, evident that Cullen was an intelligent admirer of the Venusian bard, and entered warmly into the sentiments, didactic, moral, and æsthetic, which that skilful observer of mankind and their manners has painted so truly and accurately.

It was characteristic of the same disposition, that he placed over the front door of the house, when he took possession of Ormiston Hill, from the eleventh of the First Book of the Epistles of the same author, the words, "*EST ULUBRIS*." The Rev. Mr Cameron, the former minister of the parish of Kirknewton, the author of the old Statistical Account, assigns as the reason for this inscription the fact, "that everything about the place was in such a ruinous state, so comfortless, and so unpromising, that he placed over the front door of the house" the words now quoted. The House may have been ruinous and comfortless; and the general aspect of the place may have been unpromising; but this was not the meaning of Dr Cullen, when he affixed the words *EST ULUBRIS*. Mr Cameron has altogether mistaken both the original meaning of these words and the application by Cullen. To any one who understands the lines, and the Epistle, at the close of which these words are introduced, it must be superfluous

to observe, that what Horace meant was, that discontent and dissatisfaction, restlessness and murmuring, are not alleviated or removed by changing place and residence; but that in the most deserted and solitary places, such as was Ulubræ, an obscure town in the marshy districts of Latium, if the mind is right, tranquil, and properly trained, happiness is within every one's reach. The lines are :—

“Coelum non animum mutant, qui trans mare currunt;  
Strenua nos exercet inertia: Navibus atque  
Quadrigris petimus bene vivere. Quod petis hic est;  
Est Ulubris, animus si te non deficit æquus.”\*

As if he had said, “You need travel neither to France, to Spain, to Germany, nor to Italy, in steamships or by railway, to seek to live properly. What you seek is here, at Ormiston Hill, or wherever lot or circumstances may place you, if the well-balanced mind fail you not.”

These are probably small matters. Yet they may not be wholly insignificant, if they illustrate the habits and character of a man such as was Dr Cullen.

While we contemplate this history of rational and sober relaxation from the cares of active life, one unpleasant thought arises and disturbs all. It is not a matter of doubt, that considerable sums of money had been spent in reclaiming Ormiston Hill from a state of nature, in improving it, and making it what it was left by Dr Cullen. More, indeed, was spent upon it than there could be any well-founded expectation of seeing returned during the lifetime of Dr Cullen.

In the hands of the present possessor, Ormiston Hill is still more highly improved and more completely and profitably utilized, than it was nearly seventy years back. But upon this subject it is quite unnecessary to say anything.

#### NOTE Z, p. 679.

Repeated reference was made in the first volume (pp. 110, 262, and 461) to the late Dr John Fleming, long a member of

\* Q. Horatii Flacci Epistolarum. Lib. I. Epist. XI. Ad. Bullatium. Lin. 27.





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